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Dating the Hissar Sequence—the Indian Evidence

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IN a recent number of ANTIQUITY (1) Prof. V. Gordon Childe discussed Mr Donald McCown's study of the Iranian prehistoric sequence published last year by the Oriental Institute of Chicago (2), and in his article raised the question of the dating of the Hissar settlements, for which McCown argues a higher antiquity than that hitherto assigned. The dating of such a site must necessarily depend ultimately on correlations made with the centres of civilization to the southwest, but I hope to show that synchronisms and parallels may usefully be made from the opposite direction, and from that viewpoint in which the Middle East has to be studied as geographically the Middle West.

Apart from the Harappa Culture of the Indus Valley and the Punjab, with its famous brick-built cities, its undeciphered script and its individual glyptic art, the prehistory of western India and the Iranian borderlands has been an uncomfortable enigma to the majority of archaeologists owing to the combined causes of insufficient excavation, inadequate publication, and the inaccessibility of the actual finds to the western world, concentrated as they are in local site-museums or in the Central Asian Museum in New Delhi (3). The accident of war has enabled me to study most of this material at first hand, and in the notes which follow certain points bearing on McCown's thesis and Childe's commentary are given, with the arguments, especially those concerned with stylistic considerations, necessarily in an abbreviated form. Full publication of the evidence for the Indian sequence, and a detailed examination of the relations between prehistoric India and the lands to the west must await the end of the war.

We first find clear evidence of the extension of the northeastern Iranian cultural cycle as far east as India in the pottery of McCown's ' Hissar Culture ', represented in

[1] ANTIQUITY, 1942, XVI, 353–58.

[2] *The Comparative Stratigraphy of Early Iran* (Oriental Inst. of Chicago, 1942).

[3] The main publications, cited in subsequent footnotes by the abbreviation given in brackets, are as follows :—J. Marshall, and others, *Mohenjo-daro and the Indus Culture* (*MIC*); E. Mackay, *Further Excavations at Mohenjo-daro* (*FEM*); M. S. Vats, *Excavations at Harappa* (*EH*); A. Stein, *An Archaeological Tour in Waziristan and North Baluchistan* (*WNB*); ibid. *An Archaeological Tour in Gedrosia* (*G*); N. C. Majumdar, *Explorations in Sind* (*ES*); E. Mackay, *Chanhu-daro Excavations* 1935–36 (*CDE*). The first Mohenjo-daro report was published in London, but the remaining works were published in India by the Archaeological Survey, the last three constituting nos. 37, 43 and 48 of its *Memoirs*, save for Chanhu-daro published by American Oriental Society, Newhaven, 1943.

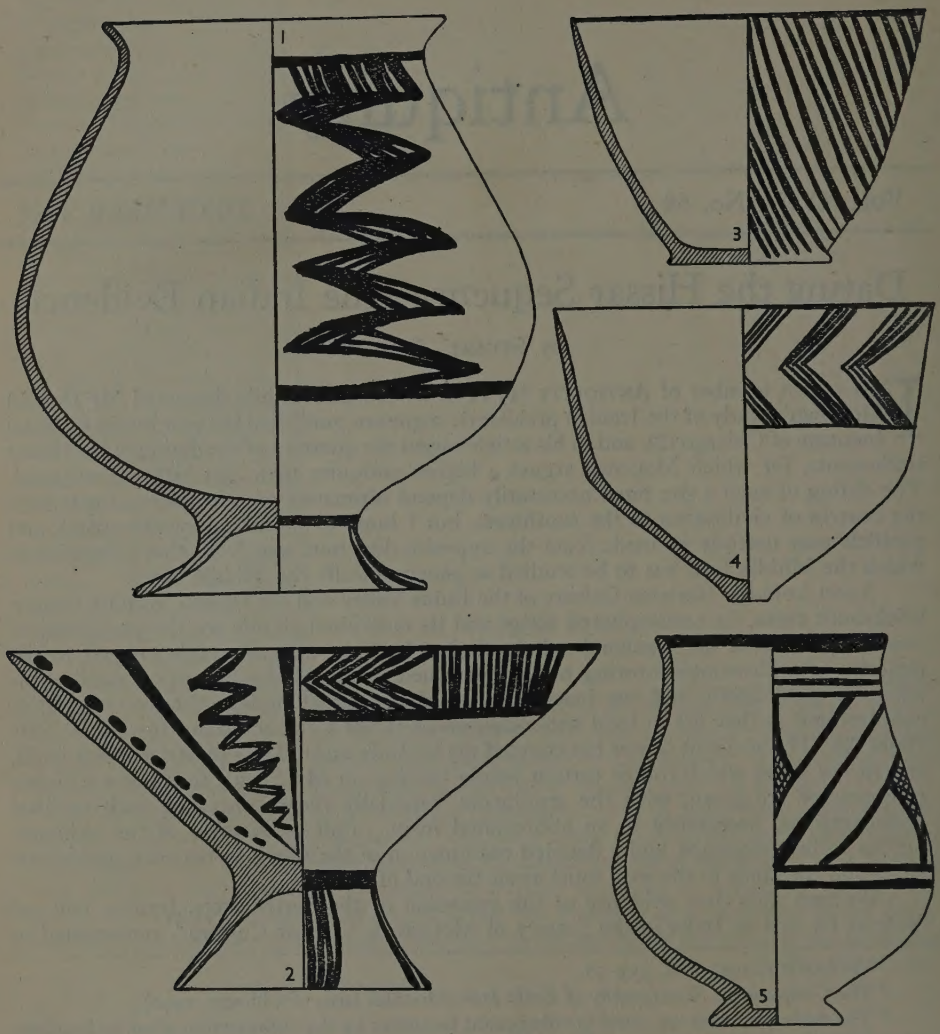

FIG. 1. VESSELS OF HISSAR Ic AND OF THE ZHOB AND AMRI CULTURES
1–4, Hissar; 5, Periano-ghundai

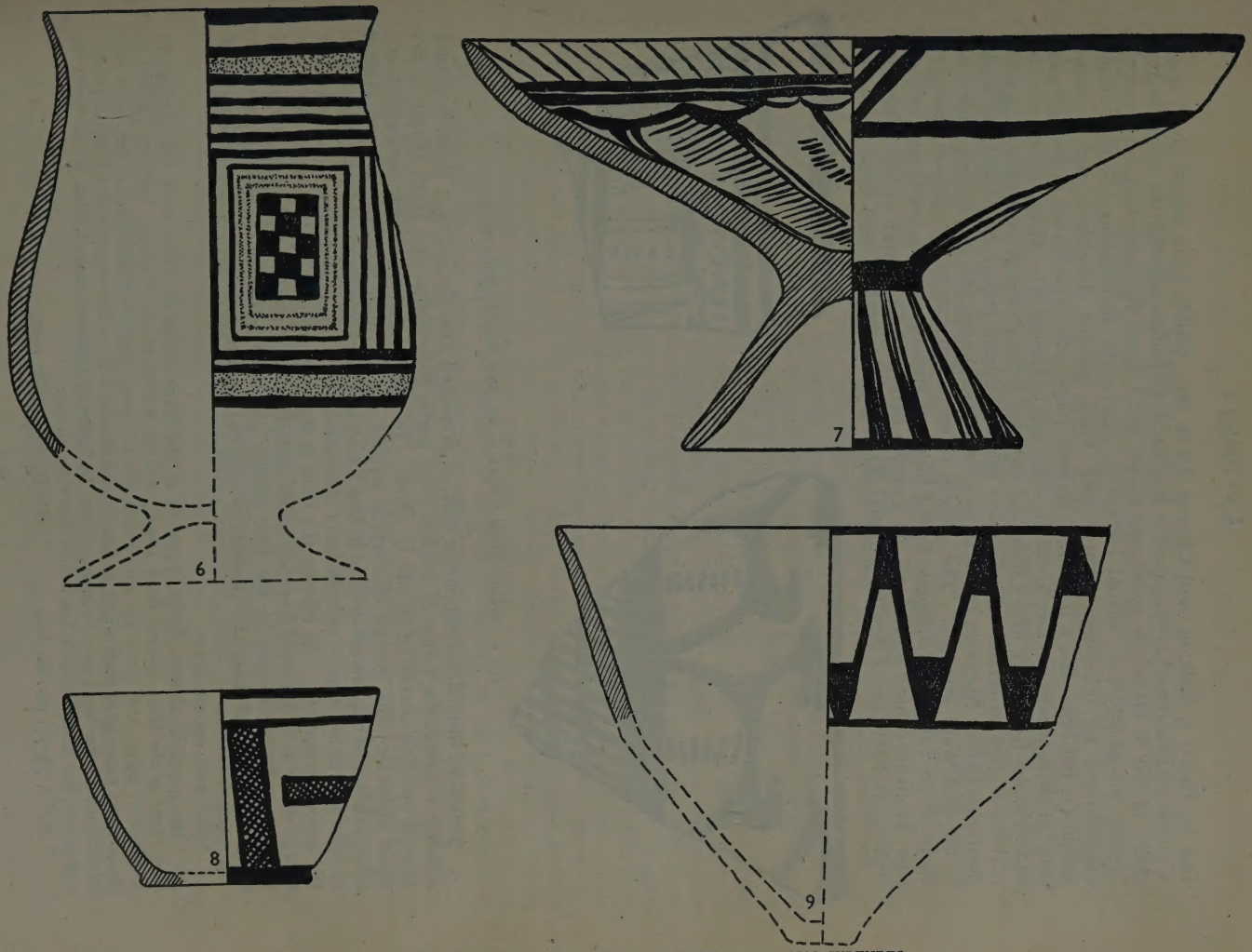

FIG. 1. VESSELS OF HISSAR Ic AND OF THE ZHOB AND AMRI CULTURES
6, Lohri (Amri Ware); 7–9, Sur Jangal

Iran by Hissar I, Sialk III, and Chesme Ali IB (4). Childe (who almost alone among Western prehistorians has examined at first-hand the Indian material) (5) notes the probability of this in his article mentioned above, and I think the suggestion can be carried beyond probability into virtual certainty. The north Baluchistan 'Zhob Culture', characterized by red-slipped wares with black painted decoration and sometimes additional red lines, has a distinctive stylistic phase best represented at Sur Jangal, a small settlement near Loralai, about 80 miles east of Quetta (6). Here the ware is buff, and although frequently with a deep red slip, this is by no means invariable, suggesting perhaps a stage in ceramic development when buff-ware influences possibly of Sialk I derivation (which McCown traces as far east as Anau) were still imperfectly assimilated in the Red-Ware province. The pottery forms which can be recognized among the sherds from Sur Jangal comprise footed bowls or shallow chalices, small cups with flat base, probable conical bowls, and tall S-profile flasks, which appear at the related but stylistically later site of Periano-ghundai (7) as globular beakers on a foot.

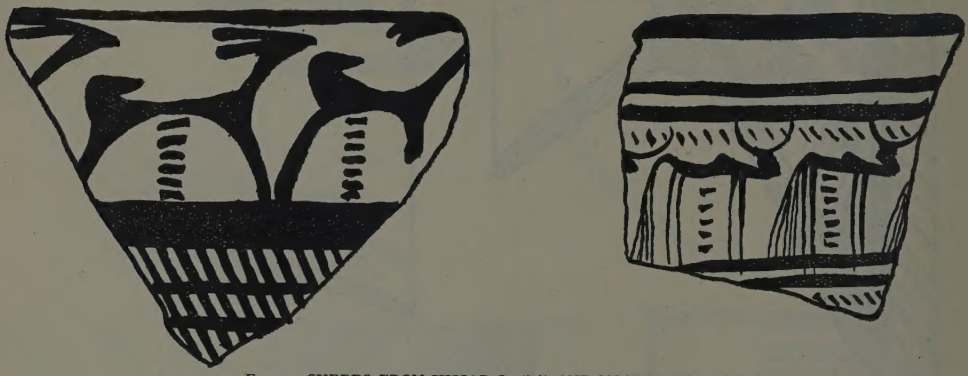

FIG. 2. SHERDS FROM HISSAR Ic (*left*) AND MOGHUL-KILA (*right*)

It will be immediately apparent that, as presented visually in FIG. 1 there is virtual identity between the forms of vessels from north Baluchistan and from Hissar I, the nearest parallels occurring in the Ic phase. In ornament, the stylistic similarities are again apparent—the groups of vertical strokes on the high pedestal feet are indeed identical. And that we are dealing in India with a parallel development from an identical continuum, rather than a second-hand derivation from the Hissar region, is shown by the animal ornament. At Hissar, the ibex is the cult animal depicted on every other sherd, while at Sur Jangal humped cattle already appear as the emblem of prehistoric India, yet their treatment by the vase painter points to a common tradition—note the tendency to vertical elongation, already present in Hissar Ic and reaching absurd lengths in

[4] McCown, op. cit. 5–13.

[5] His paper in *Ancient Egypt*, 1933, 15 ff, the main points of which were summarized in *New Light on the Most Ancient East*, 269 ff, and his lecture to the Warburg Institute printed in ANTIQUITY, 1939, XIII, 5–15 are still practically the only general treatments of the Indus and Baluchistan material in their wider archaeological setting. It will be noticed that I attach greater importance to the local sub-divisions of the Baluchistan Cultures than Childe found space to emphasize, but detailed study has forced this view on me.

[6] *WNB*, 73–77, pls. XX, XXI. [7] *WNB*, 33–41, pls. V–IX.

IIa, also at work in north Baluchistan, and in both regions the convention of a row of strokes beneath the belly of the animal (FIG. 2). The ' unilateral ladder ' pattern of McCown, used at Hissar to form panels between ibex-figures, seems at Sur Jangal to have become incorporated with the animal's legs. Of course, in the Sur Jangal phase there are other elements (e.g. an almost Halafian polychromy) which owe nothing to Hissar traditions; but such a substantial measure of agreement exists in essentials as to justify us in claiming the Sur Jangal phase of the Zhob Culture as a local manifestation, parallel to that of Hissar I, of an underlying homogeneous culture stretching from the Caspian to the northwest highlands of India.

But what is the chronological position of Sur Jangal ware, relatively to the Baluchistan–Indus sequence and absolutely, in terms of Mesopotamian chronology? Here unfortunately direct stratigraphical evidence in north Baluchistan hardly exists. I am indebted to Mr Basil Grey (**8**) for notes on material recovered from an exposure in the Rana-ghundai mound in 1940, where it seems that Sur Jangal ware was found near the bottom of a 40-foot high ' tell ' in a definite stratified layer, above which were at least four successive occupation levels the uppermost of which showed Harappa influence in the pottery. This evidence of considerable priority over the Harappa Culture, while not conclusive in itself, receives indirect confirmation from Sind. Here the existence of an early buff-ware with painted geometric ornament has been recognized for some years since Majumdar's discoveries on the type-site of Amri (**9**), where the stratigraphical priority of this ware to the Harappa Culture was first determined. Later discoveries substantiated this relationship, and it is significant for our purpose to note that Amri ware, while its decorative motifs appear to relate in the main to the Ubaid and Susa I aspects of the Iranian buff wares (**9A**), has as its dominant form a globular beaker, while from Amri itself and from Pandi Wahi come fragments of pedestal feet ornamented in a manner reminiscent of Sur Jangal (**10**), and suggesting connexion with that cultural province.

To have established priority of the wares allied to Hissar I over the Harappa Culture is not, however, to have given them any very definite chronological connotation until we can make a satisfactory equation of the Indus cities with the sequence in Sumer; for, although we have a relative abundance of objects recognized as of Indian origin in Mesopotamia, no clear statement has been made in the light of recent knowledge as to exactly what this signifies in terms of precise chronology. The importance of defining the position more clearly in our present enquiry is obvious when we pursue the Hissar sequence after the first occupation, since a double-spiral copper pin typical of Hissar II (**10A**) occurs in a Harappa context on one Indian site, and an allied single-spiral pin on another, while types characteristic of Hissar III can be recognized in post-Harappa strata there and elsewhere.

It must be said at the outset that no unambiguous Sumerian imports can be recognized from the excavated sites of the Harappa Culture. The correspondences noted by Mackay and others (**11**) seem on the whole too generalized to equate the culture conclusively with any definite stage of the Sumerian sequence, and a common origin in one or

[8] *In litt.* 1942. The sherds were found by Major M. F. C. Martin and are in the British Museum.

[9] *ES*, 24–33, pls. XVIII, XXXVIII.

[9A] Speiser has compared Amri ware with that from Gawra XIII, ANTIQUITY, 1941, XV, 164.

[10] *ES*, pl. XVIII, 13 and unpublished sherds in Central Asian Museum.

[10A] *CDE*, 195.

[11] e.g. *MIC* and *FEM*, *passim*, ANTIQUITY, 1931, V, 459–73.

other or both branches of the Iranian painted pottery cultures might give rise to a number of not very precise resemblances of the type we actually see. But south Baluchistan has provided evidence which I think gives a very satisfactory status in chronology to one of the local cultures of that region, and one against which the Harappa Culture can be assessed.

In the Kolwa and Mashkai regions of Baluchistan an individual culture, characterized by settlements with stone-built houses, cremation cemeteries, an abundance of clay figurines and a very distinctive pottery style, can be recognized and best named from the settlement-site of Kulli (12). The unstratified material recovered from these sites by Stein is confused by a mixture of definite Harappa elements in pottery and other artifacts: to this we will return later, but here it may be noted that the stylistic development which can be observed in the local painted ware implies an early stage with no Harappa influence and a later coarsening of the style and the adoption of certain new motifs and pot forms of Harappa derivation. In its stylistically early forms the painted ornament consists typically of a naturalistic frieze encircling the vessel, in which two large, elongated animals, usually humped cattle but sometimes felines, stand between trees in a landscape which may also contain subsidiary caprids on the ground and conventionalized birds and once at least a fish in the ' sky '. The design is painted in black, the animals filled with stripes or cross-hatching, but red is often used in broad bands above and below the frieze (13). Now this naturalistic landscape-with-animals motif is completely akin, in composition and spirit, and to some extent in technique, to a series of scenes on vessels from sites in Elam and Sumer—Susa, Tepe Aliabad, Tepe Khazineh, Mirvali in central Luristan, and finally from Khafaje and Tell Agrab in the Diyala Valley, where they are described by Frankfort as ' Scarlet Ware ' (14). In these examples the designs are in polychrome, red paint being used on the animals' bodies, while the beasts are usually ibexes (though there is a bull at Tell Agrab, and others at Susa); so that we seem to have again, as with Hissar I and Sur Jangal, parallel expressions of a common theme rather than direct derivation one from another, but what is of first-class importance is that Scarlet Ware is accurately dated to the first of the three sub-divisions of the Early Dynastic period recognized by Frankfort. An Early Dynastic date for Kulli Ware seems then reasonably secure on the ceramic evidence alone, but there is an additional piece of evidence pointing in the same direction (FIG. 3).

From two sites of the Kulli Culture (Mehi and the Shahi-tump settlement) and from sites showing the influence of its pot painting (e.g. Bampur in Persian Makran and the Seistan sites) come fragments of vessels made either of steatite and similar stones or of hard grey pottery imitating stones, and ornamented with incised patterns, one of which

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[12] *G*, 118–27, pls. XXI–XXIII (Kulli; 154–163, pls. XXVII–XXXI (Mehi), and minor sites *passim*.

[13] e.g. *G*, pl. XXIII, Kul. VII, 1; XXX, Meh. IV, II (cattle); XXIII, Kul. VII 4 (feline). The animals represented on Nal ware (H. Hargreaves, *Excav. in Baluchistan*, 1925, (Arch. Survey Mem. 1929), and *G*, *passim*) are in a totally different technique. I regard Nal ware as derived with Amri from a common south Baluchistan stock but evolving, through a Nundara phase, to the elaboration of the type-site. Amri ware, cut off from the Nal developments by the hills, retained its primitive character of geometric and non-representational ornament intact.

[14] McCown, op. cit. 46–7 with refs. I have not entered here into the connexions with the ' Susa II ' styles at large, the Indian features in which were noted by Frankfort in 1932 (*Arch. and the Sumerian Problem*, 69–71). He has also suggested an ultimately Indian influence at work in Scarlet ware (*Ill. Lond. News*, 6 Nov. 1937), and some cultural continuity with the Musyan area is of course implicit in the rows of caprids on Kulli ware.

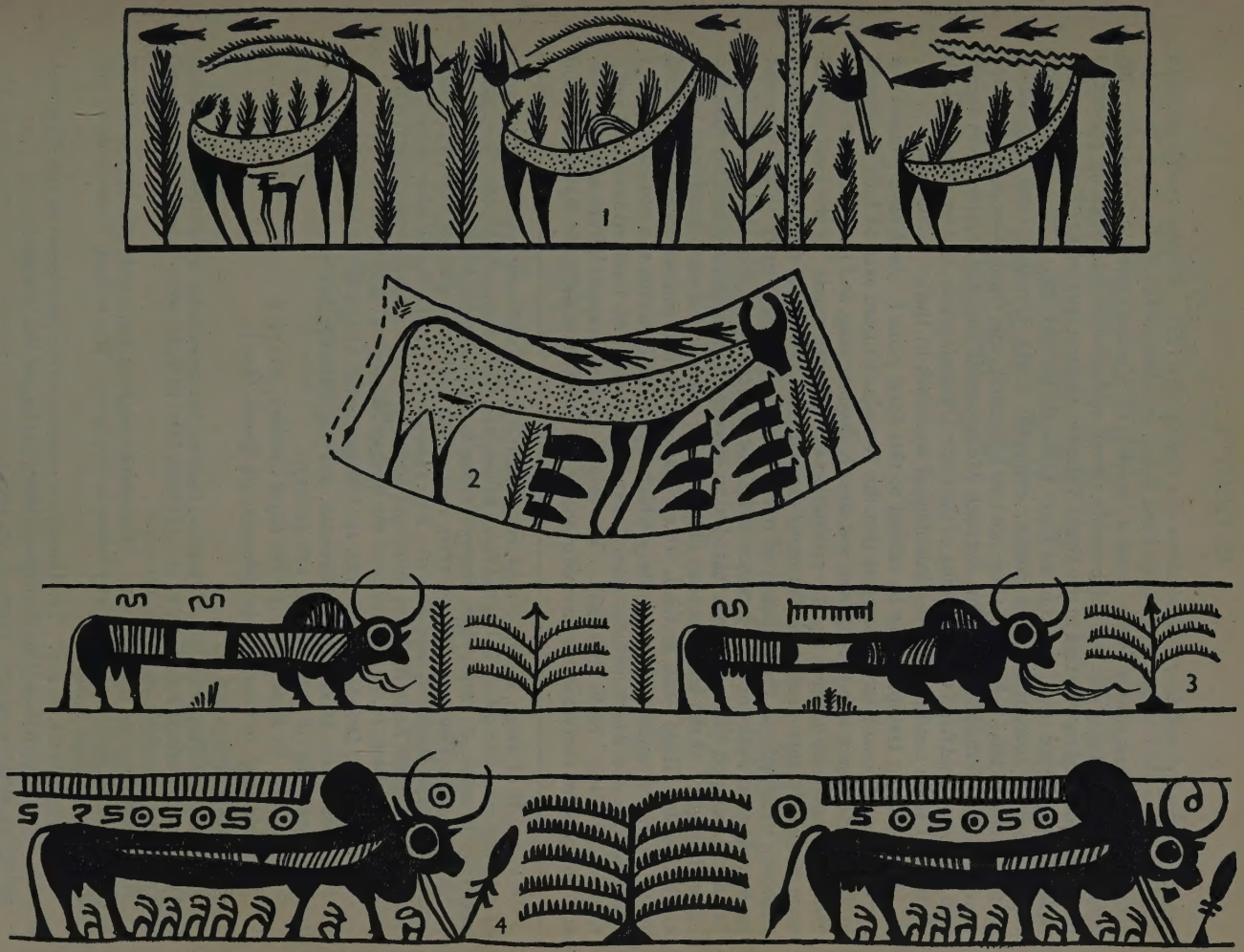

FIG. 3. THE 'ANIMALS AND LANDSCAPE' MOTIF
1, Tell Agrab; 2, Susa; 3, Kulli; 4, Mehi

represents the stylized door and windows of a circular hut (**15**). Similar vessels occur in Mesopotamia—the ' house-urns ' have been found at Khafaje (**16**) (in an Early Dynastic context) and at Susa (in the vague ' Susa II ' levels) (**17**) ; in the Queen's Grave at Ur (**18**) were two steatite cups, one with elaborately carved surface-decoration exactly paralleled by an unassociated surface find in the Dasht Valley in Makran (**19**), and at Kish fragments of similar cups were again found in an Early Dynastic context (**20**). Two other steatite cups have been recognized from Sumer, evidently Early Dynastic, and originating in the Makran (**20A**). Finally, there are two unlocated cups from Mesopotamia in the Louvre (**21**). Substantial contacts between Mesopotamia and the Makran seem therefore to have existed from the beginning of the Early Dynastic phase, and we may use Kulli Ware with some confidence as a chronological landmark in Indian prehistory.

But how can Harappa ware be related to this fixed point ? As I have pointed out above, stylistic considerations suggest that the Harappa Culture arrived fully developed in south Baluchistan when Kulli ware was already degenerating, and its appearance seems likely to be marked not only by the production of ' offering stands ' in the local painted ware but by the introduction of the motif derived from the leaf of the Pipal tree (*Ficus religiosa*), which adorns the branches of trees that in stylistically ' early ' Kulli ware and its scarlet ware analogues were rather spiky abstractions. And on one sherd, from Mehi, the humped bull is shown tethered to an object which can hardly be other than the ' sacred brazier ' of the Harappa seals (**22**). These implications seem confirmed by the evidence from Ghazi Shah in Sind, where Majumdar's excavations revealed, stratified above Amri ware, black-on-red pottery which typologically seems the earliest phase of Harappa ware and which includes several sherds decorated with humped bulls and spiky trees in the ' early ' Kulli style (**23**). The origins of the Harappa Culture as a recognizable entity seem therefore, on ceramic grounds, not to antedate the Early Dynastic period

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[15] Mehi—*G*, pl. XXVIII, Mehi, I, 6, 4, pl. XXX, Mehi II, 1, 3 (and additional unpublished examples all stone and some unfinished, in Central Asian Museum) ; Shahi-tump—*G*, pl. XIII, Sh. t. III, 9 (pottery) ; Bampur—A. Stein, *Arch. Reconnaissances in* NW. *India and* SE. *Iran* (1937), pl. VIII, IX, XXXI (pottery) ; Katukan—ibid. pl. XXXII, 12 (pottery) ; Khurab—ibid. pl. XXXII, 13 (stone) ; Seistan—A. Stein, *Innermost Asia*, pl. CXIII, RR. III.015, VII.01, III.011 and other unpublished fragments in Central Asian Museum (stone and pottery). There is an unlocated sherd, almost certainly from Seistan, in the material from the defunct Museum at Quetta, now in Delhi.

[16] H. Frankfort, *Oriental Inst. Comms.* 19 (Chicago, 1935), 53 and fig. 56 (stone).

[17] G. Conteneau, *Manuel d'Arch. Orientale*, 276 ; E. Mackay, ANTIQUITY, 1932, VI, 356–7 (stone) ; *Mem. Deleg. en Perse*, XIII, fig. 116 (pottery).

[18] L. Woolley, *Ur Excavations*, II—*The Royal Cemetery*, pl. 178, a.

[19] Unpublished, in Central Asian Museum (ex Quetta Mus.).

[20] ANTIQUITY, 1933, VII, 84.

[20A] (1) From Telloh, with 'house' pattern, undated but presumably temp. Gudea or earlier. G. Cros, *Nouvelles fouilles de Tello*, p. 41 ; (2) from Mari in Syria, with ' house ' pattern, dated as Early Dynastic. *Syria*, 1935, XVI, XXVII, 3.

[21] Conteneau, op. cit. II, figs. 447–8. The steatite cup from Tell Agrab, although carved with an Indian humped bull, does not strictly belong to this group. It is of Early Dynastic date. (*Ill. Lond. News*, 12 Sept. 1936).

[22] *G*, pl. XXX, Mehi, II, 4, 5. The Harappa connexions of the pipal leaves on this sherd were noted by Frankfort (*Arch. and the Sumerian Problem*, 71) but not the ' brazier '.

[23] *ES*, pl. XXVI, 12, 22, 26 ; XXVII, 26, 37, 38, 43.

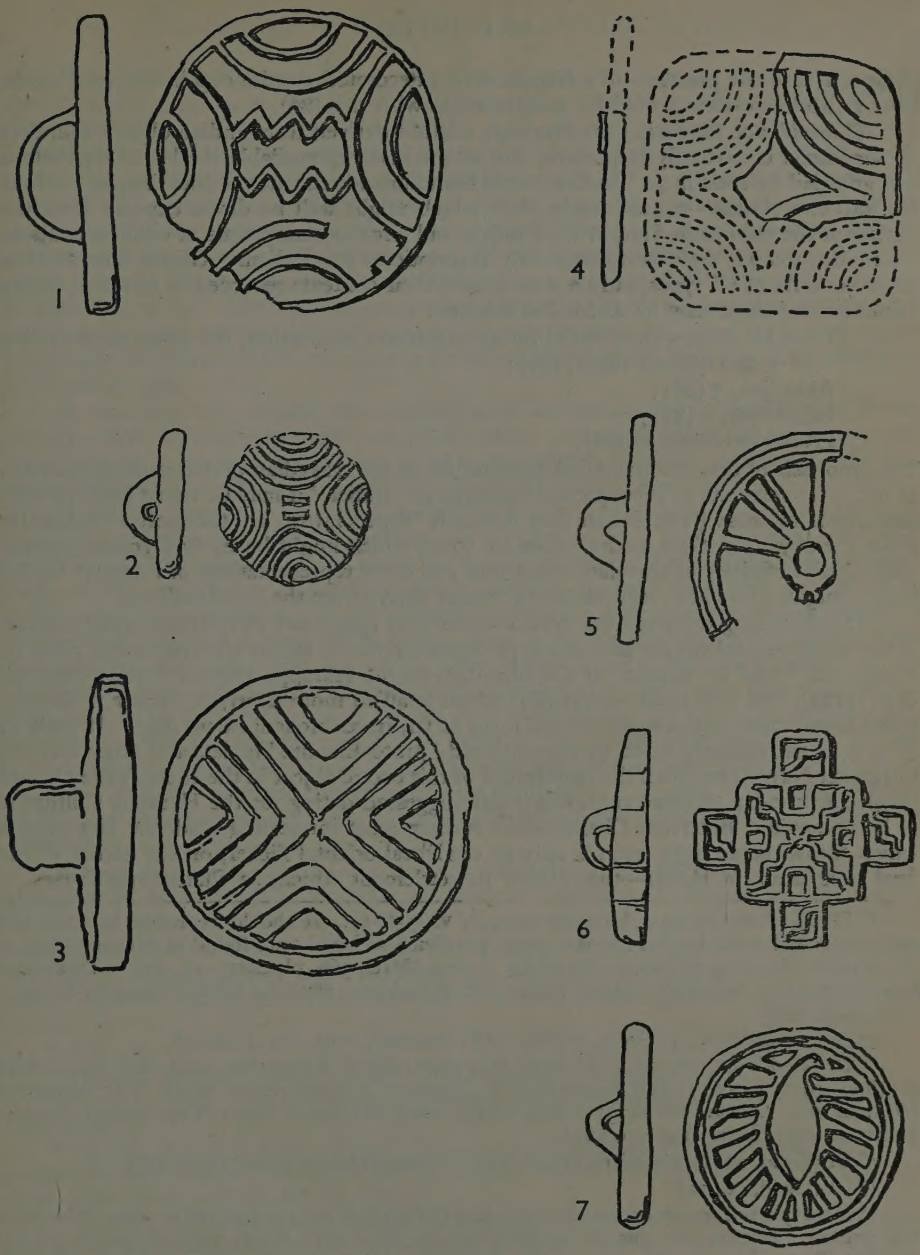

FIG. 4. COMPARTMENTED AND ALLIED SEALS: 2 FAIENCE, THE REMAINDER COPPER OR BRONZE
1, Shahi-tump Cemetery; 2, Chanhu-daro II (faience); 3, Hissar IIB; 4, Anau III; 5, Anau III; 6, Hissar IIB; 7, Susa

of Sumer, and the presence of a fragment of a decorated steatite cup in the very lowest level at Mohenjo-daro would be in agreement with this (24).

The evidence for dating the Harappa Culture derived from Indian objects found in Mesopotamia is naturally important, and of the material available the least debatable is that afforded by seals in an ' Indian ' style found outside the area of their origin. These fall into two classes—square stamp seals which might well be direct exports from the centres of the culture in Sind or the Punjab, and circular stamp seals or cylinder seals on which the motifs, style and frequently inscriptions are Indian. Of the sum total of 30 or so seals of all types, only a dozen have their contexts recorded in terms of dating evidence, and these may be divided as follows :—

Pre-Akkadian, 2 (one dated by its cuneiform inscription, the other in the filling of a II Dynasty tomb) (25);
Akkadian, 7 (26);
Isin-Larsa, 2 (27);
Probably Kassite, 1 (28).

The emphasis of this chronological distribution on the Akkadian period is most marked ; of the other elements in Mesopotamia claimed as ' Indian ' it may be noted that the least ambiguous of these (Tell Asmar and probably Tepe Gawra) are themselves Akkadian, while the objects at Susa in the ' Susa II ' strata which might be Early Dynastic, and at Tell Agrab definitely of that date—figurines and other representations of humped Indian bulls—might, as we have seen, as well or better derive from the Kulli Culture.

The foregoing excursus has been necessary to appreciate the chronological horizon of the double-spiral copper pin, of a type characteristic of Hissar IIB–IIIA and of Sialk IV, which was found by Mackay at Chanhu-daro in the Harappa Culture levels (Chanhu-daro I) (29), and of a single-spiral pin, which is allied more nearly to Hissar II double-spiral forms than to the single-*looped* form of III, from Mohenjo-daro (30). If Sialk IV is, as it seems, securely dated by its contained ' proto-Elamite ' tablets and other features to Jemdet Nasr times, then the appearance of this exotic object in the Indus Valley would involve a time-lag of some centuries if our suggested dating for the Harappa Culture is to be accepted. But from Chanhu-daro further most interesting evidence is available which throws some light, even if only an equivocal oriental flicker, on the dating of the final occupation of Hissar—that Hissar III settlement which, as Childe emphasizes, is

[24] *FEM*, pl. CXLII, 45 ; ANTIQUITY, 1932, VI, 356–57. It should be noted however that compartmented stone boxes of types closely paralleled at Mehi were found at Mohenjo-daro in late contexts, in room 76, house XIII of the VS area (*MIC*, 369, pl. CXXXI, 36, 37). These may however relate to reflex movements from south Baluchistan after the establishment of Harappa settlements there.

[25] Both from Ur, C. J. Gadd, in *Proc. Brit. Academy*, XVIII, nos. 1 and 16.

[26] Ur (Gadd, loc. cit. no. 15) ; Kish (a) (*Journ. Royal Asiatic Soc.* 1925, 697–701) ; Kish (b) (ibid. 1931, 593–6) ; Tell Asmar (a) (H. Frankfort, *Cylinder Seals*, 305) ; Tell Asmar (b) (*Oriental Inst. Comms.* 16, 1933) ; Susa (*Mem. Deleg en Perse* II, 129) ; Tepe Gawra (Speiser, *Excav. at Tepe Gawra*, 1935, p. 163).

[27] Ur (Gadd, no. 6, Frankfort, op. cit. 305) ; Lagash (*Rev. d'Assyr*, XXVII, 177).

[28] Ur (Gadd, no. 12).

[29] Mackay's full report on his Chanhu-daro (*CDE*) had not reached India when this paper was written. Interim accounts are in *Arch. Survey India Ann. Report* 1935–36 (1938), 38–44 ; *Bulletin, Boston Museum Fine Arts*, 1936, XXXIV, 83–92 ; *Journ. Royal Soc. Arts*, 1937, LXXXV, 527–45 ; *Ill. London News*, 14 and 21 Nov. 1936. The pin in question is illustrated in *CDE*, pl. LXXV, 6.

[30] *FEM*, 539, pl. C, 4.

of such importance, not only in the general archaeology of Iran and India, but for any consideration of the movements of the Aryan-speaking peoples into these lands. Of the three settlements superimposed at Chanhu-daro the first (which I propose calling Chanhu-daro I) was of the Harappa Culture, the last (Chanhu-daro III) a small settlement of people using a local incised grey ware (Jhangar ware); but between the two was a most interesting occupation distinguished from the underlying Harappa settlement not only by a distinct variant in painted pottery, but with a completely individual style in circular stamp-seals and novel metal-types. To this Chanhu-daro II occupation Mackay, relating it to an earlier find of Majumdar's (31), gives the name of the Jhukar Culture, and in it we find several objects giving suggestive correlations with Hissar III, and with these we may again link the well-known socketed axe-adze or mattock found by Mackay in his previous excavations at Mohenjo-daro in the superficial stratum, and accurately paralleled at Hissar III (32).

In assessing the cultural and chronological relationships of Chanhu-daro II the pottery, while its antecedents are not wholly clear, gives indications in agreement with other features. It does not appear to be a descendant from Harappa ware—the buff colour presents a contrast to the deep red slip of the earlier phase, and its use of red as a second colour, frequently in broad horizontal bands, suggest descent from the Amri tradition, while the plant motifs might be a degeneration from Kulli. Mackay has cited south Baluchistan parallels for the black and red chevrons (e.g. Zayak) (33), and the analogous polychromy of Sur Jangal may also be noticed—on the whole the pottery seems likely to be a descendant of the Baluchistan buff-wares with a rather mixed lineage of styles which, distinct in their early stages, seem to have coalesced in their latter days. A movement eastwards into the Indus from Baluchistan in the waning days of the Harappa Culture seems likely enough, and in passing one may note that the grouped designs of cattle and other animals on the pots from the 'H' cemetery at Harappa (34), belonging to the latest phase of the site's history, are almost certainly derived, though at a very long remove, from those on Kulli ware, and reinforce the idea of a movement of mixed cultures eastwards, perhaps partly to acquire new and fertile land from a civilization no longer strong enough to resist, partly impelled by the first intruders from the steppes to the north and west (35).

The stamp-seals of Chanhu-daro II mark a complete break with the Harappa tradition and must be as intrusive as the other elements of the culture. In general their origins seem to lie with the great family of Iranian 'button-seals' going back to Hissar I at least, and one sees the survival of early types (e.g. a close comparison can be made between a very simple type of pottery seal from Hissar IIA and one from Chanhu-daro II) (36) but I would draw attention to one circular faience seal from the Indian site, with the field

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[31] *ES*, 5 ff.

[32] *FEM*, pl. CXX, 27; Schmidt, *Excav. Tepe Hissar*, 1937, 204–5.

[33] *G*, pl. I, Z.N. 7. The applied incised cordons on this sherd are probably to be considered a late feature.

[34] e.g. *EH*, pl. LXII, 1, 5, 11. Note the degenerated flanking trees on 11, and the filling of the background with 'sigmas' and other motifs. For the man between two animals cf. a 'Susa II' sherd, *Mem. Deleg. en Perse*, XIII, fig. 177.

[35] Burials of the same culture as the 'H' Cemetery were found high up at Mohenjo-daro, e.g. that with a vessel paralleling *EH*, pl. LXI, type J. (*MIC*, 323, pl. LXXXIX, 2).

[36] Schmidt, op. cit. pl. XXVIII, H. 1785 (Hissar IIA); *Ill. Lond. News*, 21 Nov. 1936, 908 fig. 1 right (Chanhu-daro II); *CDE*, XLIX, 2.

divided into four quarters of curved concentric lines (**37**); a design parallel in some detail on the copper stamp seals from the Shahi-tump cemetery in Makran (**38**). Now these seals belong to a class of copper seals which I propose calling the ' compartmented ' group (FIG. 4), in which the pattern on the face is built up of ' compartments ' bounded by metal strips, and outside Shahi-tump the type and its related forms appear to have an interesting distribution in space and time. At Hissar we may note two copper seals —one, from Hissar IIB, circular, with quartered pattern (**39**), the other from IIIB, cruciform but in ' compartmented ' technique (**40**). The latter seal has been compared by reason of its outline to one from Anau III (**41**), and in this occupation at least two ' compartmented ' seals were found, one showing the curved quartering of the Shahi-tump and Chanhu-daro examples (**42**). And at Susa an analogous seal was found in the mysterious ' deuxième niveau ' but apparently associated with a sherd having an incised design of a boat with crescent-standards which at Lagash appears to date to the time of Gudea (**43**). Finally, the Shahi-tump cemetery, despite the ambiguity of its archaistic pottery, has an approximate *terminus a quo* provided by the find of a piece of a clay toy cart (**44**) of Harappa type in the Kulli occupation into which the cemetery was dug, while the heavy shaft-hole axe found with one of the burials (**45**) may be compared with another from Chanhu-daro II (**46**). Approximate contemporaneity seems likely therefore, on the grounds of the ' compartmented ' seals and their analogues, between Chanhu-daro II, the Shahi-tump cemetery, Anau III and Hissar III, while the Susa evidence, if it is to be relied upon, would give a correlation with Mesopotamia in post-Akkadian times. And the correlation Chanhu-daro II–Hissar III is strengthened by the presence, in the Indian site, of the pin with unilateral-looped head which makes its first appearance in Hissar IIIB (**47**), and the find of the copper mattock mentioned above at Mohenjo-daro in a post-Harappa context.

The foregoing evidence suggests that in India a parallel sequence obtained to that in northeastern Iran, and that we could make a correlation which would run :—

(i) Hissar I	Zhob Culture
(probably, especially IC)	(Sur Jangal ware)
(ii) Hissar II	Harappa Culture
(probably the end of the phase)	(Chanhu-daro I)
(iii) Hissar III	Jhukar Culture
	(Chanhu-daro II)

Expressed in terms of Mesopotamian chronology, we have seen that the evidence implies that stage (i) is earlier than Early Dynastic but probably later than Ubaid or Susa I;

[37] In the Central Asian Museum.

[38] *G*, pl. XIV.

[39] *Museums Journal*, Philadelphia, XXIII, pl. CVII, a.

[40] Schmidt, op. cit. fig. 118, H. 2697.

[41] McCown, op. cit. 60.

[42] R. W. Pumpelly, *Explorations in Turkestan* (Washington 1908), I, figs. 256, 258.

[43] *Mem. Deleg. en Perse* I, fig. 353. For the sherd cf. H. Frankfort, *Studies in Early Pottery*, I, 69 and cf. *Mem. Deleg. en Perse* XXV, fig. 79 (attributed to III Dynasty).

[44] *G*, pl. XIV, Sh. T. II, 12.

[45] The published photograph gives the misleading impression of a flat axe. (*G*, pl. XIII, St. T. VII, 135).

[46] *CDE*, pl. LXXII, 25; *Ill. Lond. News*, 21 Nov. 1936, 909 fig. 5.

[47] loc. cit, fig. 11; Schmidt op. cit. pl. LIII, H. 3141.

(ii) is not earlier than Early Dynastic and may have its maximum expansion in Akkadian times and (iii) is not earlier than Akkadian and probably some centuries later. But this would not be in accordance with McCown's chronology, in which Hissar III ends in Akkadian times, and in confronting this contradiction there are two points which we must take into consideration. The first is the time-lag which may have delayed the arrival of objects, particularly metal types, with a western point of origin, at their final Indian resting-place. After all, our stage (ii) correlation rests on a single copper pin (and a probable analogue) of the very type which makes a belated appearance in the Early Bronze Age of countries west and north of Hissar, and the archaistic survival of certain imported metal tools in India is implicit in the socketed axes of the Shahi-tump cemetery and Chanhu-daro II, which are basically Early Dynastic forms (48).

The second point is the possibility of correlations being made between Hissar III and the Harappa Culture. At Hissar itself a cylinder-seal was found engraved with a figure of a bull which might be reasonably enough considered as vaguely Harappa in origin (49); the etched carnelian beads might link up with the east as well as the south-west, and the curious animal on the 'Dancer's Necklace' styled by Schmidt a lion, might be a dewlapped bull with a perforated hump for suspension, while the stepped turquoise bead in the same find carries out a favourite Harappa motif (50). The flat gold beads with central tube (which occur, as McCown points out, at Ur from Early Dynastic III to Akkadian times) occur in metal and in faience in the Indus Valley (51), and multiple-string necklaces with space-beads occur both at Hissar and in the Harappa Culture. Finally the mysterious 'miniature columns' might be compared with the rather similar object from Kulli (52) and with various baetyls in the Harappa Culture usually included in the phallic series, and there is a possibility that the copper 'mace-head' from Hissar III (53) may have more than a fortuitous resemblance (upside down) to the 'unguent bottle' of the same metal from Chanhu-daro I (54).

Objects in the Harappa Culture of possible Hissar III derivation are not perhaps so easy to identify, but the animal-headed pins from Mohenjo-daro and Harappa (55) are certainly suggestive of the Hissar 'wands', and in the Khurab cemetery in Persian Makran, not precisely dated but I think likely to be Early Dynastic to Akkadian and showing Kulli influences. Stein found a curious copper rod surmounted by the figure of a camel which again may be compared with the Hissar III objects (56). The correlation made by McCown between the cruciform seals of Hissar III and Anau III might be taken a stage further to include one from Harappa (57), and Childe has pointed out the general resemblances subsisting between Anau III and Harappa in e.g. metal types and cart

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[48] The Chanhu-daro axe can in fact be paralleled accurately in the *Royal Tombs at Ur*—Woolley, op. cit., pl. 223, type A 11.

[49] Schmidt, op. cit. fig. 118, H. 116.

[50] ibid. 223–6.

[51] *MIC*, pl. CXLVI, 34 and 38; *EH*, pl. CXXXIII, fig. 3.

[52] *G*, pl. XXIII, Kul. I, X, 1.

[53] Schmidt, op. cit. pl. LII, H. 771.

[54] *Bull. Boston Mus. Fine Arts* XXXIV, 83–92, fig. 9; *Ill. Lond. News*, 21 Nov. 1936, 909, fig. 8; *CDE*, pl. LXXIII, 39.

[55] *FEM*, pl. C, 4, 10; *EH*, pl. CXXV, 36.

[56] Stein, *Arch. Reconnaissances in north-western India and south-eastern Iran*, pl. XVIII, E, 1, 258.

[57] *EH*, pl. XCI, 253.

models (58), while the spiral-headed pin from Mohenjo-daro might be considered as nearer the Hissar III single-looped type than the Hissar II double spiral.

But are these resemblances of any greater significance than those subsisting between the Harappa Culture and Sumeria which we have dismissed as imprecise and inclusive? The objects of possibly Harappa derivation at Hissar provide after all little more than a *terminus a quo* for their arrival in northeast Iran, while animal-headed pins appear to go back to Uruk times at Susa (59), so that their appearance may in India and at Hissar be unrelated save by reason of a remote common origin in an earlier cultural stratum. On the whole, the Indian evidence, incomplete and in some respects obscure as it is, seems fairly consistent both within its own cultural provinces and in its outside contacts, and to argue in favour of a low, rather than a high, dating for Hissar III. That it is wholly conclusive is not for a moment claimed: my object in this paper has been to show that it should not however be ignored, and that it can be used in chronological discussions even in the present imperfect state of our knowledge. In any schemes for post-war archaeology in the East, the further elucidation of the Indian prehistoric sequence should be given an important place, not merely to establish this within India, but as a means whereby the stratigraphy and chronology of Iranian sites can be checked from the east no less than from the west. Sites such as Dabar-kot (60), with a Harappa occupation as the final settlement of a 100-foot high ' tell ', must contain an antecedent series of immense potentiality, and at Mehi the exact chronological point of impact of the Harappa Culture in south Baluchistan should be ascertainable. And at the latter end of the scale, the easterly movements of peoples from Iran into India, and from Baluchistan to the Indus (hints of which we have noticed above) should be given precision, and the makers of the clearly intrusive coarse, unpainted ware often with applied bands of finger-tip ornament which appears on the surface of certain north Baluchistan ' tells ' might be identified (61). Then only might we see a hope of dawn in that ' prehistoric night ' in which Childe so charmingly pictures the Vedic hymns being sung.

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[58] *New Light on the Most Ancient East*, 280.

[59] *Mem. Deleg. en Perse*, XXV, 197 and fig. 34, McCown, op. cit. 55.

[60] *WNB*, 55 ff.

[61] e.g. *WNB*, pl. X, M.M.d. and unpublished material collected in 1942 from Kaudani and adjacent sites by Mr J. Reid Dick. The remarkable ' encrusted ' ware from Dabar Kot (*WNB*, pl. XV) should also be noted.

La date des kourganes de Trialeti

par C. F. A. Schaeffer

LES étonnantes trouvailles retirées par la mission russe de Tsalka (direction B. A. Kuftin) des grands tertres funéraires ou kourganes de la vallée supérieure de la Khram dans la province de Trialeti, Géorgie centrale, à l'Ouest de Tiflis (1), ont causé une sensation dans le monde des archéologues (2). Comme, à l'occasion d'une étude d'ensemble sur la chronologie du Bronze au Caucase (3), nous venons d'étudier ces nouvelles trouvailles, il nous paraît utile de publier ici nos conclusions relatives à la date qu'il convient d'attribuer au kourganes de Trialeti. La question, en effet, intéresse au plus haut point l'archéologie du Caucase.

Disons tout de suite que Mr. Kuftin considère l'époque de la quatrième tombe à fosse de Mycènes et celle de Troie VI comme le *terminus post quem* des plus récentes des kourganes qu'il classe à la fin du Bronze Moyen. Il ne croit pas devoir envisager pour eux une date postérieure aux XVIIe siècle avant notre ère, tandis que nous serions en faveur d'une attribution de l'ensemble de ces kourganes aux XVIe–XVe s., c'est-à-dire au début du Bronze Récent.

Chacun des 42 kourganes examinés de Trialeti n'abritait qu'un seul corps ; ce sont donc certainement des tombes de chefs locaux. La richesse des mobiliers funéraires, d'ailleurs, le confirme. L'extrême rareté, dans la plupart des kourganes l'absence complète, d'armes atteste qu'il s'agit de chefs d'une population paisible. A en juger d'après les offrandes déposées dans les kourganes, celle-ci se livrait à l'élevage de bovidés, moutons, chèvres, et cochons ; en plus, elle pratiquait la chasse à la chèvre sauvage, au bouquetin, au chamois, au chevreuil, au daim et cerfs et au sanglier. Parmi les nombreux animaux offerts en sacrifice funéraire, le cheval n'est pas représenté. Les chariots à quatre roues en bois plein déposés dans certains des kourganes sont dépourvus de leur attelage. L'un semble avoir été tiré par des boeufs.

Le fer fait complètement défaut parmi les objets du mobilier funéraire ; à l'exception des flèches taillées en obsidienne ou en silex, tous les objets métalliques sont en bronze ou en métal précieux.

La question de la date de ces trouvailles est compliquée par le fait que, ni pour les étonnants produits métalliques, ni pour la belle céramique peinte des kourganes de Trialeti, on ne saurait citer des pièces analogues provenant du Caucase même ou des pays immédiatement voisins. D'un autre côté, comme Mr. Kuftin l'a signalé (l.c. p. 164),

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[1] B. A. Kuftin, *Archaeological Excavations in Trialeti*, I, *Academy of Sciences of the Georgian* SSR. *The Institute of History*, Tiflis, 1941.

[2] Dans le dernier numéro d'Antiquity, No. 67, Sept. 1943, p. 129 ff. le professeur Ellis H. Minns, a consacré à l'ouvrage de Mr. Kuftin un compte-rendu détaillé, illustré de 46 figures ; le lecteur voudra bien s'y référer pour l'ensemble des découvertes de la mission de Tsalka. Nous ne nous occupons ici que de la question de la chronologie des kourganes attribués au Bronze Moyen.

[3] Elle fera partie d'un volume intitulé *Eléments de Chronologie de Ras Shamra*, en préparation pour l'Institut Griffith de l'Université d'Oxford.

les rapprochements avec l'orfèvrerie sumérienne et celle d'Uruk du IIIe millénaire, en dépit de certaines ressemblances de détail, ne sont pas concluants. Les profondes dissemblances techniques et de style écartent toute possibilités de rapport direct. Il en est de même de la comparaison que l'on pourrait, à première vue, être tenté d'établir entre la céramique rouge peinte en noir des kourganes et celle de l'Elam dite du second style.

Seul le fait de l'absence du cheval parmi la faune reconnue à Trialeti et la présence dans certains des kourganes de ce type archaïque de char à quatre roues en bois plein, parlent apparemment en faveur d'une antiquité reculée de ces trouvailles (Kuftin, l.c. p. 160).

En ce qui concerne le premier de ces arguments, la suite des fouilles à Trialeti pourrait le rendre inopérant. Rappellons à ce sujet, que jusqu'aux trouvailles récentes de Tell Ahmar (Til Barsib) et de Mari, le cheval (4) a été considéré comme inconnu en Mésopotamie avant le milieu du IIe millénaire. A Trialeti, il y avait peut-être des raisons d'ordre religieux ou simplement matériel ou sentimental qui interdisaient le sacrifice du cheval à titre d'offrande funéraire. Notons que dans les tombes de guerrier, mêmes les plus riches, de l'époque de la Tène en France et en Allemagne du Sud, où les chariots avaient été déposés avec le harnachement complet, il est extrêmement rare que les chevaux aient été sacrifiés.

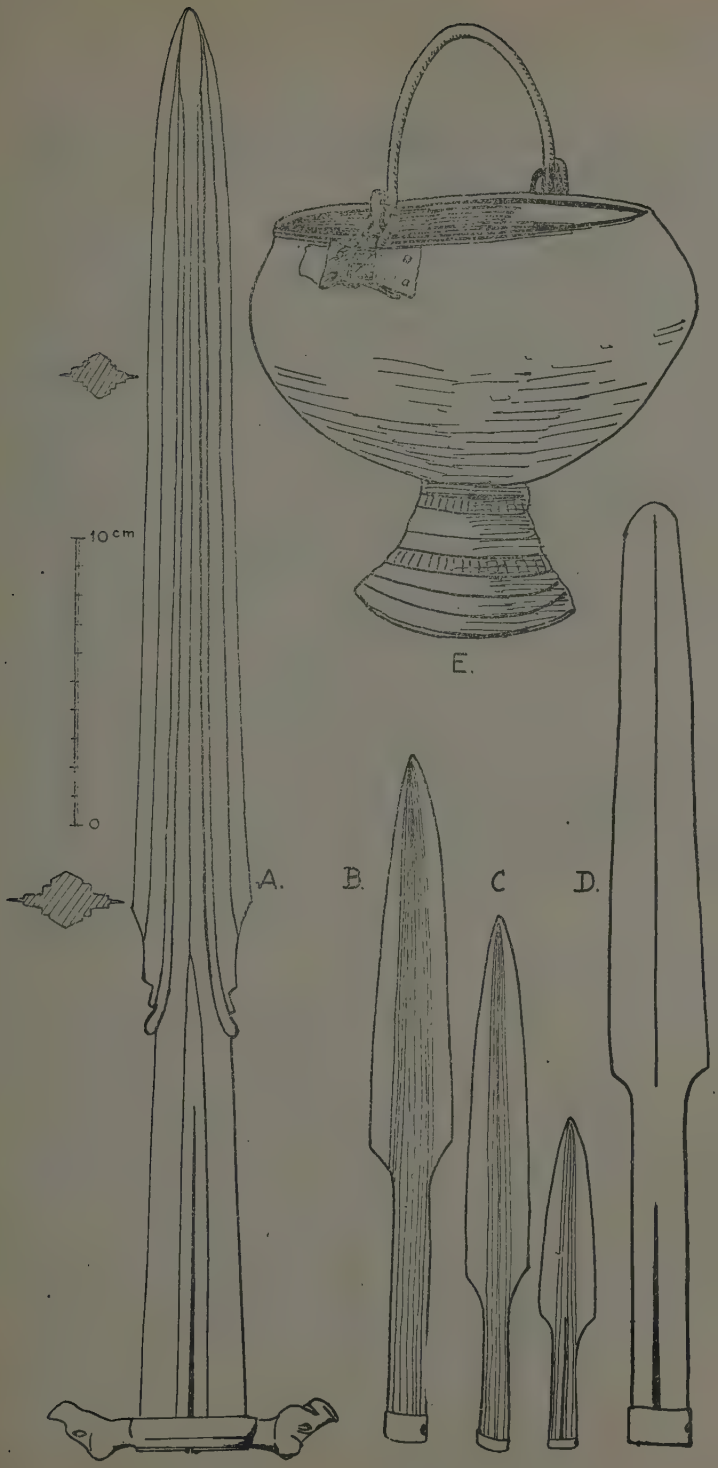
Quant aux chars à roues en bois plein, on en rencontre qui circulent encore de nos jours dans les pays montagneux de l'Asie Antérieure.

Le problème s'éclaire dès qu'on envisage pour les kourganes de Trialeti une date dans les limites du Bronze Récent. A ce sujet, la découverte dans le kourgane XV d'une tête de lance en bronze à douille muni d'un anneau de serrage en argent (FIG. 1, B), est particulièrement décisive, comme Mr. Kuftin, l'a bien noté (l.c. p. 165). L'auteur semble admettre que la lance à douille n'apparaît dans la zone de la Méditerranée orientale et en Asie Antérieure qu'à partir de l'époque des tombes à fosse de Mycènes et de Troie VI. Il est possible de nommer plusieurs sites et, en particuliers, Chagar Bazar et Ras Shamra-Ugarit, où des lances à douille avec anneau de serrage ont couramment été utilisées à partir de 2000 en chiffres ronds (FIG. 1, D de Ras Shamra, type du XIXes).

La lance de Trialeti ne remonte cependant pas à une date aussi reculée. Cela est prouvé par le décor particulier de la douille, primitivement probablement conçu comme un renforcement, et consistant en une série de godrons en relief. Ils se prolongent le long de l'arête médiane de la lame pour converger un peu en avant de la pointe (FIG. 1, B). Une lance de ce type, aux godrons plus saillants, a été trouvée à Ras Shamra où elle date de l'Ugarit Récent 1 ou du début de 2, c'est-à-dire de la période entre 1550 et 1400 avant notre ère (FIG. 1, A). Une comparaison plus frappente peut être établi entre la lance de Trialeti et celles tout à fait semblables retirées d'une tombe intacte de la colline de Kephalari au voisinage de Mycènes (FIG. 1, B, C,). La céramique de cette tombe est du type du Helladic Récent 3 rencontrée en abondance aussi à Ras Shamra et appartient à la période entre 1450 et 1350 en chiffres ronds. La douille proportionnellement plus courte des lances de Prosymna (5) et leur anneau de serrage plus faible, pourraient typologiquement indiquer une date légèrement plus récente par rapport à celle de la lance de Trialeti. Par ailleurs l'analogie est telle qu'elle oblige d'attribuer ces armes à la même période. Nous proposons donc pour la lance du tertre XV de Trialeti la période entre 1550

[4] Distinct de l'onagre utilisé comme animal de trait dès le IIIe millénaire comme les trouvailles d'Ur le prouvent.

[5] C. W. Blegen, *Prosymna*, tombe X, pl. 127, fig. 510.



A, RAS SHAMRA (1550–1400)
B, TRIALETI, KOURGANE XV
C, PROSYMNA
D, RAS SHAMRA (XIX^{e}S)
E, TRIALETI, KOURGANE XV

et 1400 en chiffres ronds. Cette date est valuable pour l'ensemble des riches kourganes de Trialeti lesquels, à en juger d'après la répartition des divers genres de céramique dans les mobiliers funéraires, doivent appartenir à la même période.

A l'appui de l'attribution proposée par nous pour les trouvailles de Trialeti, on peut citer divers indices. D'abord, d'une façon générale, l'extrême rareté des armes qui contraste avec la richesse des tombes. On la constate dans le mobilier funéraire de la même période dans beaucoup de pays voisins du Caucase. Elle correspond à la stabilité relative de la situation politique générale dans les pays de l'Asie Antérieure qui caractérise cette période. En ce qui concerne le décor de la poterie peinte de Trialeti, l'emploi fréquent du motif de la spirale isolée ou conjugués s'accorde bien avec la prédilection qu'on avait pour cette combinaison décorative à l'époque mycénienne (Kuftin, l.c., pl. LXXVII–LXXVIII). Sur le gobelet historié en argent du kourgane V (Kuftin, l.c. pl. XCI ; Minns, l.c. ANTIQUITY, September, 1943, p. 131, no. 43), devant le personnage assis, est figuré, en plus d'un tabouret tripode ou autel flanqué de deux chèvres, une sorte de vaisseau élancé. A en juger d'après ses dimensions et les détailes gravés près de son bord supérieur, il pourrait représenter un tambour. Or des vaisseaux de ce type et de cette taille ont été retrouvés dans les tombes de la nécropole de la fin du Bronze et du début du Fer de Beshtasheni fouillée aussi par la mission de Tsalka (Kuftin, l.c. pl. LIII), nécropole que nous avons des raisons d'attribuer entre 1300 et 1100 en chiffres ronds. Un autre détails qui établie un rapport entre les trouvailles des kourganes de Trialeti et celles des nécropoles postérieures du Bronze et du Fer de la Transcaucasie est le décor du beau gobelet en or trouvé dans le kourgane XVII (Kuftin, l.c. pl. XCIII, Minns, l.c. p. 131, no. 36). On rencontre les mêmes spirales pendants vers le bas et dans une technique similaire, en grènetis, sur la gaine de l'un des poignards du début du Fer (1200–1000) de Mouci-Yeri trouvé par de Morgan (*Miss. au Cauc.*, pl. III). Une date dans les limites de la seconde moitié du IIe millénaire est indiquée aussi par l'anse du seau en argent serti d'or du kourgane XVII (Kuftin, l.c. pl. LXXXVIII ; Minns, l.c. p. 131, no. 38). Elle présente la même technique que celle des torques à torsade du début du Fer trouvés par de Morgan au Lelvar (1200–1000).

L'étonnant récipient en bronze battu posé sur un pied creux et muni d'une anse mobile du kourgane XV (Kuftin, l.c. pl. LXXXVII, ici fig. 1, E) rappelle si vivement les chaudrons en bronze du début du Fer de l'Italie méridionale et septentrionale que l'on pourrait éprouver quelque hésitation à lui assigner une date remontant aux XVI–XVe siècles, comme l'on est obligé de le faire à cause du contexte archéologique. Les disques en or au décor repoussé tel que celui du kourgane XVIII (Kuftin, l.c. pl. CIII) doivent être comparés aux pendentifs analogues provenant des tombes à fosse de Mycènes (Karo, *Schachtgraeber*, pl. XXIX), de Ras Shamra (nos *Cuneiform Textes of Ras Shamra-Ugarit*, pl. XXXII, fig. 1) et de la Perse (Morgan, *Dél. Perse*, VII, pl. XII, fig. 127) qui chronologiquement se placent entre le XVIe et le XIVe s. avant notre ère. Les pointes de flèche en silex et obsidienne des kourganes de Trialeti à entaille triangulaire à la place du pédoncule (Kuftin, pl. CVI bis) sont à tout égard identiques aux flèches trouvées dans les tombes indiscutablement de la fin du Bronze ou du début du Fer de la Transcaucasie Orientale (cf. par ex. *Eur. Sept. Ant.*, VIII, 1933, p. 119) et du Talyche (Véri, Tulu, Djonu, cf. Morgan, *Préh. Orient.* III, fig. 202). Enfin les poignards à soie en argent ou en bronze, aux gouttières si élégamment tracées, des kourganes XVII et XIX de Trialeti (Kuftin, l.c. pl. CV, CIX ; Minns, l.c. p. 131, no. 37), sont très nettement les prototypes immédiats des poignards analogues, mais techniquement inférieurs de Beshtasheni de la fin du Bronze (1300–1100). Le même type de poignard, qualitativement très près de ceux des kourganes, a été retiré des tombes aux squelettes contenus dans des urnes de la nécropole

d'Esery en Abchasie attribuée par nous à la période entre 1550 et 1450 (cf. *Eur. Sept. Ant.* VII, 1932, p. 98; l'attribution de cette nécropole par Mr. Ivascenco à l'époque de la colonisation grecque a déjà été refutée par Mr. A. M. Tallgren, cf. *Minns Vol. Eur. Sept. Ant.* IX, 1934, p. 25, 29). Ajoutons que la présence dans les tombes d'Esery de très belles lances à douille est en contradiction avec l'opinion exprimée dans le rapport de Trialeti (Kuftin, l.c. p. 165) suivant laquelle les lances à douille en bronze n'auraient fait leur apparition dans le Caucase septentrional qu'à partir de la fin du Bronze.

Il ressort des rapprochements précédents que l'étonnant ensemble de trouvailles provenant des kourganes de Trialeti ne remonte pas plus haut que le milieu environ du IIe millénaire, mais aussi qu'il faut reviser l'opinion du fouilleur suivant laquelle il n'y aurait pas eu de rapport entre la civilisation reflétée par le contenu des kourganes et celle des nécropoles du Bronze final et du début du Fer mises au jour ailleurs dans la province de Trialeti et dans la Transcaucasie en générale. Ainsi, pour expliquer les différences de l'une par rapport à l'autre, il n'est pas nécessaire d'admettre un hiatus prolongé entre elles. Au contraire, les rapprochements que nous venons de relever permettent de raccorder les trouvailles des kourganes, en dépit de leur indéniable originalité et supériorité qualitative à l'ensemble des produits de la civilisation du Bronze Récent de la Transcaucasie.

D'un autre côté, dans l'orfèvrerie de Trialeti réservée à l'usage des chefs enterrés seuls dans ces impressionnants kourganes (hauts jusqu'à 5 m.) des influences étrangères peuvent être identifiées. Ainsi, Mr Kuftin a relevé une certaine ressemblance des perles creuses en or ornées de spirales en grènetis et de pierres serties en capuchon des kourganes VII et XVII (Kuftin, l.c. pl. XCIV, XCV, XCII ; Minns, l.c., p. 131, nos. 40, 41) avec les parures analogues de la tombe de Vafio (C. Tsountas, Αρχαιολογικη 1890, pl. 7). Il a insisté aussi sur l'analogie générale des tunics ou kaftans et des chaussures à la pointe relevée portés par les personnages figurés sur la coupe en argent du kourgane V (Kuftin, l.c. pl. XCI, XCII ; Minns, l.c. no 43) avec le costume hittite tel qu'il est représenté sur les bas-reliefs de Yazili-Kaia (sur la date de ces reliefs, voir en dernier lieu nos *Ugaritica*, I, p. 120). Ce rapprochement, en outre qu'il appuie la date relativement récente attribuée par nous aux grands kourganes de Trialeti, permet, aussi, de reconnaître le caractère religieux de la scène figurée sur le gobelet en question, destiné à devenir fameux dans le monde des archéologues.

Grim's Bank, Padworth, Berkshire

by B. H. St. J. O'Neil

THE immediate environs of Silchester consist of fields, which are either now under plough or else have been arable for many years in the recent past. Consequently there are few, if any, traces there of the Roman roads which led from the various gates to Dorchester, Speen and Cirencester, Sarum, Winchester, and London. A mile or more to the north and northwest of the Roman town, however, there is a belt of land, which is largely heathland except where trees have been planted. Here there are clear indications of the line of two Roman roads, one from the west gate, west–northwest to Speen and Cirencester, the other from the north gate to Dorchester (Oxon.)

The road to Speen (FIG. 1) was formerly thought to follow closely the modern road along the northern side of Silchester Common and thence to run along the straight county boundary between Berkshire and Hampshire. In recent years, however, Mr O. G. S. Crawford has shown that the road, instead of following this traditional line, ran west–northwestward to cross the river Kennet near Brimpton Mill. It is traceable as a raised camber or a deep hollow way from Catthaw Lands Copse, about half-a-mile from the west gate of Silchester, to the western side of Hungry Hill. Further west, in Decoy Plantation, and again beyond the road from Padworth Common, i.e. in Keyser's Plantation, it is clearly seen as a broad cambered way (O.S. 6-in. Berkshire XLIV, SE, Hampshire IV, SE). Beyond this point the present writer has not followed it, but Mr Crawford has noted its continuation.

The road to Dorchester left the north gate of Silchester. A short distance north of this gate, i.e. east of Stonehill Copse, it is visible as a hollow way, with a similar continuation down to West End Brook. North of this stream there is a broad causeway on the line in the flat land, followed by a hollow way up the hillside, past Lovegrove's Farm. Then for a quarter of a mile there are only faint and uncertain traces.

Beyond the next main road, however, i.e. that from Mortimer Common to Mortimer West End, the Roman road becomes visible, and from that point northwards for over a mile it is most clearly traceable with hardly an interruption. Its course here forms a valuable object lesson in understanding the directness of the layout of Roman roads. At first it is a broad camber, then for a few yards a hollow way, as it descends the slope to Pottinger's Furze, then again a camber. Its precise course on the southern slope towards the stream in Pottinger's Furze is not certain, but, once the stream has been crossed, the road appears as a camber upon a most massive causeway actually on the line of a tributary stream. The camber follows the bed of this stream, and appears on the flat land to the north not quite so prominently as further south, but still unmistakable. In Hundred Acre Piece the camber is well marked, particularly in its northern part, where young trees and heather were recently burnt (? in 1941).

The road up to this point from Silchester runs on one alignment. North of the road from Burghfield to Padworth, in The Pines, it is still visible as a camber, but not so clearly because of higher vegetation, proceeding a few degrees further west than hitherto. Its course up to this point appears on the O.S. 1-in. map (5th edition); beyond this point however it seems not so far to have been recorded.

The cambered way can be traced, although with difficulty, as far as the eastern side of Pond Slade. Within this copse the vegetation is very thick even in March, and for a

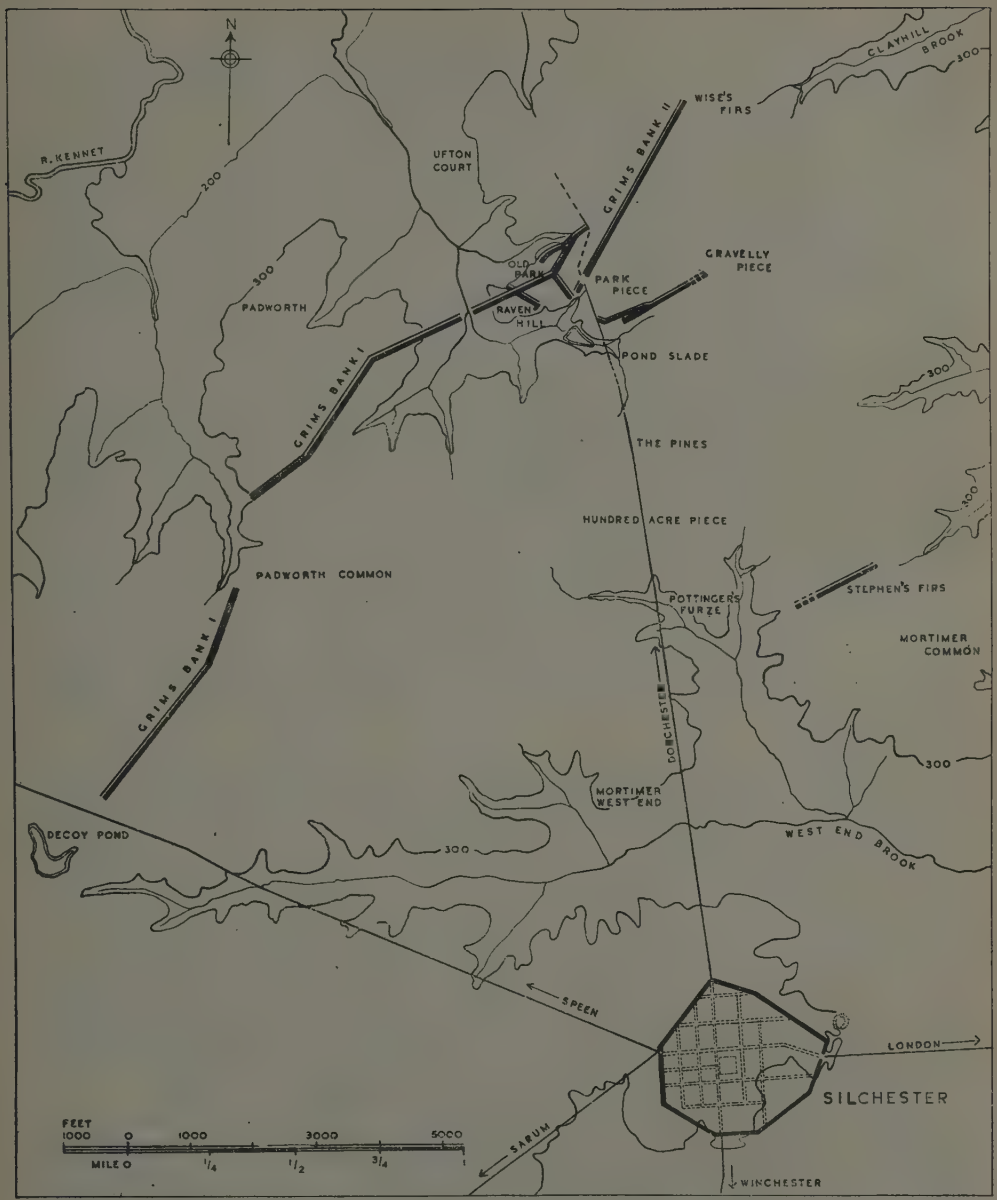


FIG. 1. SILCHESTER AND GRIM'S BANK

short distance there are no certain traces of the road. After about 50 yards, however, a well-preserved camber 20 feet wide becomes visible on the bearing 350° (1). This camber is cut by the stream in the copse, but is visible beyond it on the same alignment for a distance of 50 yards ; here it is particularly well preserved, although invisible in summer because of bracken.

Near the western side of Pond Slade this camber has been destroyed by the construction of a more recent boundary bank and ditch, which converges upon it from the south and then in all probability follows precisely the line of the Roman road northwards for 50 yards. This boundary bank and ditch continue on the same alignment across a straight track which runs northeast to southwest, and there is no certain trace of the Roman road on either side of it. It is, therefore, probable, especially as it is precisely on the line of the road further north, that it is actually on the line of the road. If so, there was a change of direction (from 350° to 341°), probably at the point where the camber becomes merged in the boundary bank and ditch in Pond Slade.

The line taken by the road necessitated another crossing of the stream in Pond Slade. It is an excellent illustration of the direct method of the Romans, that the stream was thus crossed twice, whereas a very slight deviation eastward would have reduced this natural obstacle to much smaller proportions. Except for one small mound of gravel near the southern end of the Hollow Way, which is soon to be described, there are no vestiges of the road across the marshy ground in the bottom of Pond Slade, which is here much wider than in the other crossing to the south.

On the northern side of the stream, however, there is a considerable slope up to the north in Park Piece. Here, on a bearing of 349°, there is for a short distance, i.e. for about 30 yards actually in the slope, a Hollow Way. It seems clear from the next discovery northwards that this bears some relation to the Roman road. It may not be actually the road itself, but some later variant of it. On the other hand the slight change of alignment may be only a local variation of the road itself, in order to facilitate the ascent of the slope.

At the top, i.e. the northern end of this Hollow Way, there is visible a very well-marked camber, 20 feet wide, on a bearing 342°. It is particularly noticeable in the few yards of open land south of the main area of Park Piece. In Park Piece itself, which is flat, the camber is traceable in the same manner and on the same bearing for about 300 yards, i.e. as far as Grim's Bank II. At the point of junction there is a gap in Grim's Bank II, so that without excavation it is impossible to be certain of their mutual relationship. At least, however, it cannot be assumed that the Bank disregards the road ; indeed there is a strong suggestion that the gap in the Bank is deliberate and that the road passed through it. The camber seems to exist actually between the two ends of the Bank.

Beyond this point northwards there are only very faint traces of the road for about 50 yards. Thereafter no traces at all have been found unless the bank or camber at the extreme eastern end of the Grim's Bank I (see below) represents a portion of the Roman road. This seems likely, and has been so marked on the map. The deviation to reach this line must have been designed to avoid a deep combe to the northwest.

Between this area and the river Kennet prolonged search failed to yield any traces of the road, nor was any evidence forthcoming along the hillside north of the river opposite Ufton. It is, however, probable that the crossing of the river took place just north of Ufton Green, since there is here a low ridge, possibly of glacial origin, running roughly north and south across the flood plain from the present railway to the foot of the hillside

[1] All alignments given are in accordance with true north.

at Lambden's House. In places there is a suspicion that this ridge has been artificially raised or straightened to form a causeway.

The roads from Silchester to Speen and from Silchester to Dorchester have now been described so far as is required for the present purpose. Between them at from two to three miles northwest or north of Silchester there runs a linear earthwork, called Grim's Bank. Where best preserved, towards its southwestern end, this bank (Grim's Bank 1) stands about 8 feet high above the bottom of the ditch, which is uniformly on the northwestern side, i.e. away from Silchester. Elsewhere the bank is between 3 and 5 feet high.

The southwestern end of this earthwork is to be found on Little Heath about one mile southeast of Aldermaston Court. The bank here is 8 ft. high, and the ditch wide and broad, but there is no indication that either of them extended further to the southwest. In fact this is one of the clearest cases of a well preserved original end of a linear earthwork, which has ever been seen by the writer. A slight depression, about 7 yards in diameter, close to the end of the bank may represent the site of a hut, but the indications are not convincing. From the end of the earthwork southwestwards it is 80 yards to the well-preserved camber of the Roman road from Silchester to Speen, which has already been described. It is perhaps significant that a marshy piece of land, represented by Decoy Pond, lies a short distance away on the other side of the Roman road.

For over half a mile from its beginning the earthwork runs straight to the northeast. It then turns, apparently by two very obtuse angles which cannot be separately shown on a small scale plan, to run in a more northerly direction up to the western edge of Padworth Common. It stops and seemingly always did stop about 35 yards south of the steep side of a small valley ; presumably, therefore, this land was heavily covered with undergrowth, when the Bank was made. For the next 450 yards the valley already mentioned, along with other similar natural features as shown by the contour on the map, must by the vegetation they carried have been sufficient deterrent to enemies, thus making the provision of an artificial barrier unnecessary.

The earthwork begins again in the clearest possible manner at the head of one of the small valleys already mentioned (2). The bank is 5 to 6 feet high, but the ditch is now scarcely visible on account of ploughing or deliberate levelling. East of the road from Padworth to Padworth Common the dyke for some distance has been almost ploughed out, but its track can be ascertained, and it is clear that its alignment here, as elsewhere throughout its course, is composed of straight sections. No curve occurs even in the negotiation of a steep valley.

The Bank continues as marked on the plan, although it is reduced in height to 3 feet on its way down the side of a valley due south of Ufton Court. It is not actually visible in the bottom of the valley, and may not have been constructed there, but the point could only be decided by excavation.

The Bank is present on the eastern slope of this valley and runs close to the summit of Raven Hill, as marked on the 6-in. o.s. map (edition of 1913). But that map is in error in making it run due east from this point for 300 yards. There is no bank at all

[2] A narrow trench, dug from north to south, 40 yards west of this beginning, showed conclusively that Grim's Bank had never existed at that point. There were no signs of a made bank or of a ditch. Everywhere, except as stated below, the subsoil occurs immediately below 1 ft. 6 ins. of soil and roots. The southern end of this trench cut what seems to be a bank at the edge of the field. Here the soil and roots are 2 ft. 9 ins. in depth or more and very hard at the bottom, but they are certainly due to ' soil-creep ' from the field, and were not deliberately thrown there by man.

in that position. The linear earthwork continues to run, although at most only 3 feet high, in almost the same direction as before for 300 yards through Old Park. At a point 100 yards from the western end of this section it crosses another earthwork of much larger proportions. The latter consists of a bank about 40 feet wide, standing 4 feet above natural ground to the west and 8 feet above the bottom of the ditch to the east, which is about 30 feet wide and 4 feet deep. It runs in a straight line from west-northwest to east-southeast. Its western end is within a few feet of the edge of a steep declivity to a small valley in Ufton Park, and appearances suggest that both bank and ditch always ended at this point. The eastern end of this earthwork lies close to the head of a small re-entrant combe, and the actual end of the rampart is inturned for a distance of 25 feet. This inturned entrance shows with hardly any element of doubt that the earthwork is of pre-Roman date. A survey of the land behind it, i.e. to the southwest, whether on the ground or on the map by the means of contours, shows that here indeed is a natural promontory. Only on one side was an artificial defence needed, in order to turn the area into a stronghold of normal Iron Age type. Camps of this kind are not common in the Home Counties, where suitable natural features are rare. A parallel can, however, be quoted from Keston Common, Kent (3).

The bank of the linear earthwork becomes merged at their junction in the bank of this promontory fort, but the ditch of the former has plainly been cut through the bank of the latter, since it appears to this day as a V-shaped depression therein. Moreover there is a slight bank across the ditch of the promontory fort, which can be none other than the linear earthwork itself. In a word there is at this point on the surface of the ground one of the clearest examples in the writer's experience of the visible sequence of earthwork construction. There is not a shadow of doubt that the linear earthwork succeeded the promontory fort, probably after a considerable lapse of time. This crossing of the two earthworks also clearly illustrates another aspect of linear earthworks, namely that they were not lines of defence but of demarcation. Had this earthwork been intended for defence, a portion at least of the bank of the promontory fort, which lay in front of it, would have been levelled. As at present preserved, however, this bank definitely blocks the view from the linear earthwork.

At the eastern end of the 300 yards already mentioned Grim's Bank 1 turns once more, and runs north-northeast for 270 yards ; then it turns again northeast (4), and is present for another 70 yards. At the end of this last portion it joins a broad bank or camber, which is visible north-northwestwards across the narrow combe. This camber can be plainly distinguished from the old field banks, which are marked on the 6-in. O.S. map, but no extension of it can be seen either to the north or south beyond the actual sides of the coombe. Nevertheless, as already suggested during the description of the Roman road from Silchester to Dorchester, the camber is likely to be a vestige of that road. In any case the road cannot have been very far away, and there is no indication that the linear earthwork extended any further to the east.

Consequently Grim's Bank 1 is seen to be a line of earthwork drawn athwart the approaches to Silchester on its northwestern side. Apart from any other consideration its relationship to the promontory fort on Raven Hill suggests that it is of Roman or post-Roman date, and its obvious dependence upon two main roads from that town shows that they as well as their focus must still have been in use when it was constructed. There is little doubt on general grounds that it was constructed as a frontier line after the

[3] *Arch. Cant.*, XLV, 124–8.

[4] At this point another bank, with ditch to north, becomes merged with Grim's Bank 1. It runs for about 250 yards westward ; its position is unusual and its meaning is quite uncertain.

Roman evacuation of Britain, but full consideration of this matter will be given in a later article.

At the change of direction of Grim's Bank I in Old Park between the portions measuring 300 yards and 270 yards another bank and ditch of similar proportions with ditch on its northeastern side branches off and runs in a straight line southeastwards to end on the edge of a small valley. Not far away on another part of the same valley there is the beginning of a much larger earthwork than the last. It is as big as the western end of the Grim's Bank, already described on Little Heath, and it has its ditch on the northwestern side. This earthwork, Grim's Bank II, is visible in a dead straight line for nearly three-quarters of a mile, but between the eastern side of Church Plantation and Island Farm it has been almost destroyed by cultivation. For the same reason its northeastern end cannot now be determined with accuracy, although the presence of a spring in Wise's Firs, immediately north of Island farm, suggests that the land hereabouts was thickly forested under primitive conditions. If so, the earthwork may never have been constructed much beyond its present visible end. There are three gaps in the earthwork[5]; it is likely that the Roman road from Silchester to Dorchester passed through the southernmost (see page 190), which is marked on the plan (5).

This dyke (Grim's Bank II) differs from Grim's Bank I in two respects; it is entirely on one straight alignment, and it seems to refer back not to Silchester itself but to some area east thereof. Its straightness may be explained to some extent by the nature of the ground it traverses, which is quite flat, but at the same time its character in this respect, as in its reliance upon an area east of Silchester, is shared by two other dykes further southeast.

The southern of these two is the short length of bank, with ditch to north, which is recorded on the O.S. maps in Stephen's Firs at Mortimer Common. At its eastern end it clearly rested upon the head-waters of a stream, and it is probable that its western end did likewise, but here it has long since been dug away by gravel-diggers. There is no certain ancient entrance in the part of this dyke, about two-thirds, which is still preserved.

The other dyke is much nearer to Grim's Bank II. It runs through Park Piece and Gravelly Piece on an alignment of 243°. Beginning, as far as present indications are a guide, near but not actually on the side of a steep slope, the same in which is the Hollow Way of the Roman road from Silchester to Dorchester, it runs dead straight, but for a slight irregularity of a few yards, for 200 yards to the north–south metalled road from Ufton Nervet. Where best preserved, the bank is 4 feet high. The ditch lies north of the bank; both have been interrupted by a modern gravel pit, but appear on each side thereof.

East of the metalled road the dyke is still visible on the same alignment. The bank is broader, perhaps on account of ploughing, which is attested by the indications of ridge and furrow. Detailed investigation is impossible, because the trees of Gravelly Piece, being rather young, form an impenetrable barrier. It is, however, almost certain that the dyke went as far as the eastern fence of Gravelly Piece. Beyond that there are no signs of it, just as beyond Island Farm there are no signs of Grim's Bank II. It may well be that both dykes once ended on forest or thick undergrowth, of which there is now no indication.

Just 100 yards east of the gravel pit, mentioned in the last paragraph, this dyke is

[5] Other small gaps in this and the other earthworks have not been shown on the plan, since it is impossible from surface indications to suggest whether or not they are original, and it is felt that a multiplication of gaps on the plan would be more confusing than their complete absence.

joined by another, which is quite definite and of similar character, although not very prominent. At this point the two dykes become one, and it is not possible from surface observations there to say which preceded the other. For 40 yards westward the two banks, only half coalesced, seem to share a common ditch. Thereafter for 40 yards,—and perhaps once further but the gravel pit has removed the evidence,—the two systems are separate, but the bank of the northern seems to overlie some of the ditch of the southern. Beyond the gravel pit the two banks and ditches are parallel and very close, but quite distinct. As already stated, the southern, or main, bank and ditch end a few yards further west. But the northern system goes still further west, first on the same alignment, 240°, for 142 yards from the western edge of the gravel pit, then for 23 yards on a bearing of 288°. At this point, 207 yards along the Roman road south of the northern edge of Park Piece, the bank joins that road; this is precisely at the top of the Hollow Way.

The mutual relationship of these two parallel banks and ditches is difficult to determine without excavation. The balance of probability, however, is in favour of the northern system being later than the southern. If this interpretation is the correct one, the second work seems to have been erected to connect the earlier system with the Roman road, just as the cross-bank, already described, between Grim's Bank I and Grim's Bank II seems to have been intended to bridge a gap, which might otherwise have been a break in the barrier system.

Thus, apart from Grim's Bank I, there are three straight linear earthworks, of which one, the northern (Grim's Bank II), seems to have taken account of the Roman road. The middle one also (in Park Piece) does so in its present form, but this may not be original. These dykes, however, especially the southernmost, seem not to be concerned with Silchester. As already mentioned, they refer to some other site east of the Roman town. This can by geography be none other than the hill-top now called Mortimer Common. Few objects of Romano-British times have been found there, but on the other hand there is the significant fact that there are four round-barrows in the vicinity; on Rocque's map three others are marked. This accumulation of barrows suggests that this plateau was important in prehistoric times and that it carried an early trackway. It is highly probable that in late-Roman and early post-Roman times it was still of importance alongside the completely Roman Silchester. Both areas may well have needed the barriers or frontiers provided by the linear earthworks. Those which refer to Mortimer Common probably lie across the line of a trackway, which, not being made up like a Roman road, is no longer visible.

Whether the site at Mortimer or Silchester was the first to be thus defended or defined may never be known. It is indeed possible that the two systems are contemporaneous. On the other hand the difference between them, already noted, that the eastern system is straight, whereas the western often changes direction, a difference which admittedly may be due entirely to the terrain, is reinforced by the consideration that the Grim's Bank of Silchester is single, whereas the frontier of Mortimer, although shorter, is triple. Little is known of the comparative chronology of linear earthworks, but such a difference of type does seem to be fundamental and must surely reflect some distinction in purpose, if not in date. In one respect, however, the two systems are alike; both have supplementary dykes, which at one end run on to the Roman road. Grim's Bank I has this at its eastern end, whilst one of the Mortimer dykes (in Park Piece) has it at its western end. It is at least a plausible suggestion that these short banks were added to both of two differing systems, in order to bring them into close relation as parts of a whole frontier barrier.

Note

The section shown in FIG. 2 was cut across Grim's Bank 1 nearly half a mile south-southwest of Old Farm, Padworth and 213 feet west-southwest of B.M. 310.9 on the road thence to Round Oak. The ditch could not be completely excavated, and the filling, so far as it could be observed, produced no useful evidence. The shape of the ditch has been restored according to the line of the sections dug across the similar earthwork on Greenham Common, near Newbury (6). The subsoil is composed of orange sandy gravel, 1 ft. to 3 ft. thick, below which is a stratum of sand, which was sometimes revealed, especially in the digging of the ditch. The line between the gravel and the sand is shown on the section by a broken line within the subsoil.

The bank is composed of a core of grey, sandy gravel and an upper part of dirty, yellow, sandy gravel, no doubt the product respectively of the topsoil and the subsoil

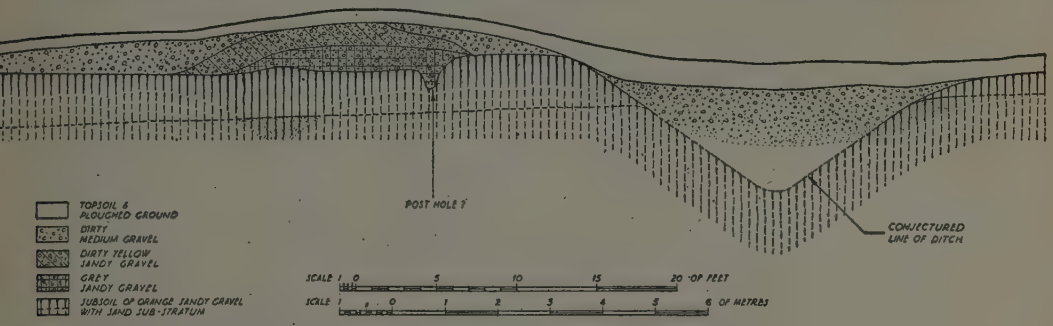


FIG. 2. SECTION OF GRIM'S BANK NEAR PADWORTH

during the digging of the ditch. Near the front of this gravel bank there is a hole in the subsoil, which careful examination suggested should certainly be considered as a post-hole, 1 ft. 6 ins. in diameter at the top. If this suggestion is correct, the position of the post-hole shows that it held one of a series of posts, which were the uprights of a timber revetment for the bank. The present position of the gravel layers at the front of the rampart may then be due to a slip forward after the decay of this revetment.

Present conditions do not allow a prolonged search for parallels to this revetment in a linear earthwork, but so far none has been found. Such a feature, however, would be an obvious measure of precaution, to prevent rapid decay of the bank in such gravelly soil. It is, of course, frequently found nowadays during the scientific excavation of the ramparts of prehistoric camps.

For assistance with this section the writer is indebted to Mr W. E. Harris.

[6] *Arch. Journ.*, forthcoming.

Harvesting Implements in Denmark

by E. Cecil Curwen

Ancient Harvesting Implements. By Axel Steensberg (Copenhagen, 1943). 4to. pp. 275. 13 folding plates and 80 figs. in text. Price not stated.

British archaeologists will be relieved to know that some at least of their Danish colleagues have been able to continue their good work during the last three years, in spite of the German occupation of their country. The fact that a sound archaeological work by a former pupil of Prof. Gudmund Hatt has been published in Copenhagen in the English language in the year 1943 is encouraging evidence of this.

In this work Dr Steensberg classifies and describes all the known specimens of flint, bronze and iron sickles, iron scythes and leaf-knives that have been found in Denmark as well as in those parts of southern Sweden which once formed part of the Danish dominions. His chronological range is from the neolithic period down to the eighteenth century, and the distributions of the principal types are shown on a series of maps. This is followed by a discussion of the origin of harvesting implements and their evolution in the Old World down to modern times. The earlier literature of the subject is reviewed at some length, and covers the principal work done in western Europe since the time of Sir John Evans. An important omission, both here and in the bibliography (which contains no fewer than 367 authors or sources) is Dr Grahame Clark's paper on British curved flint sickles (1).

In addition to all this painstaking museum and library work the author has opened up an entirely fresh line of enquiry by making replicas of several ancient types of sickle and scythe and testing their absolute and relative efficiency on the harvest field. This is a most praiseworthy innovation, and it is to be hoped that Steensberg's example will be followed by other students of implement typology. It is one thing to study a series of tools in a museum, but the attempt to gain practical experience of the actual use of such tools (or replicas of them) may well teach us a good deal that we should not otherwise learn about them, as, for instance, factors governing form, or the advantages of changes of form observed in a typological series.

Another praiseworthy feature of the book is the way in which the subject of study is followed through to modern times as a unitary whole, without a quite arbitrary break between prehistoric and historic times.

Owing to the difficulty of obtaining this book (2), which will be of great value to students of implement typology, it may be useful to give a rather fuller outline of its contents.

Flint Sickles

Dr Steensberg classifies Danish flint sickle types as follows (fig. 1) :—

Type 1. Flake-knife of flint used for reaping (reaping-knife) ; subdivided into :—

Type 1 a. Straight blade, retaining bulb ; longitudinal flake-scars on dorsum ; back often steeply chipped. Gloss, which is found on 14 of the 22 datable specimens,

[1] *Proc. Preh. Soc. E. Anglia*, 1932, vii, 67–81.

[2] For my copy I have to thank the author, as well as Dr Sigfrid Svensson, Keeper of the Nordiska Museet, Stockholm, who, acting for the British Council there, has forwarded the volume to me.

may affect both faces, extending to the back of the blade and to both ends. From this it is inferred that the blade was not hafted, but was held in the hand for reaping. Dated by associations to the Passage Grave period (before 1900 B.C.).

Type 1 *b*. A late, coarsened variant of *a*, usually characterized by a concave cutting edge. The flake of the well-known hafted sickle found at Stenild is of this type (3). These are not datable by associations, but are referred by the author to the transition between the Stone and Bronze Ages.

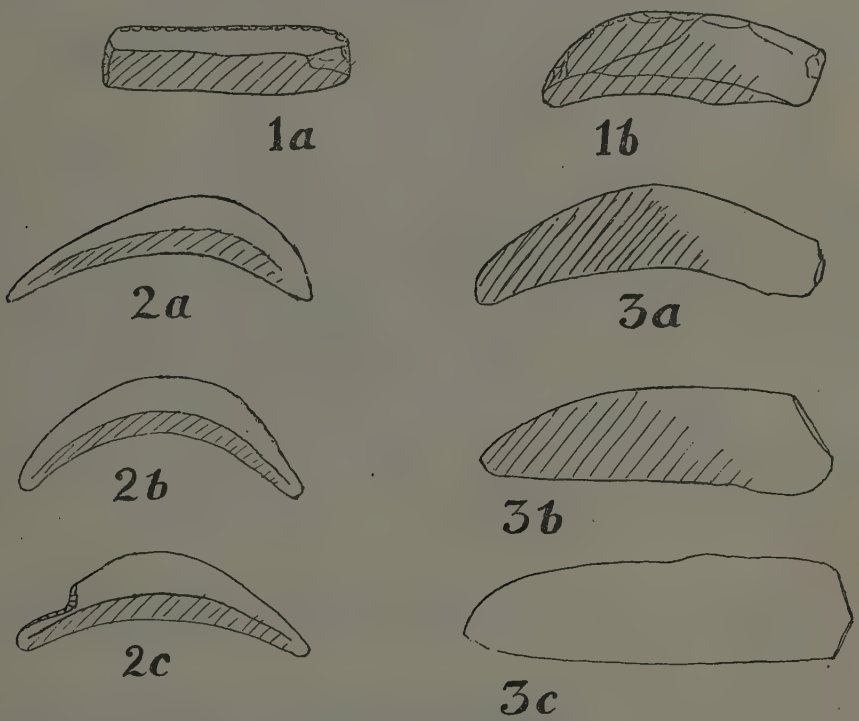


FIG. 1. TYPES OF DANISH FLINT SICKLES
All varieties of Types 2 and 3 are chipped all over both faces. Shading indicates typical glossy areas

TYPE 2. Crescentic flint implements, the entire surface worked with parallel chipping, and having a concave cutting edge. Subdivisions :—

Type 2 *a*. Asymmetrical.

Type 2 *b*. Symmetrical.

Type 2 *c*. Crescentic sickle having one end shaped by chipping into a kind of handle or tang which is secondary, and due to rehafting in a different manner (probably as Type 1 *b*).

A considerable number of specimens of Type 2 have been dated by associated finds to the Dagger period (*c*. 1800–1600 B.C.), and large numbers have been found in

[3] See photo in ANTIQUITY, 1938, XII, pl. II, following p. 152.

hoards. Of 428 specimens of Types 2 *a* and 2 *b* gloss appears on 223 (52 per cent.), nearly always extending from the concave edge up to the median bulge or keel. Of 10 specimens of Type 2 *c* gloss appears on six.

Crescentic flints having the cutting edge straight, convex, or serrated are not considered by the author to have been used as sickles, and they are therefore excluded from consideration here. I cannot help feeling very doubtful whether he is right in this view. He regards the serrated ones (' saws ') as leaf-cutting tools.

Type 3. Parallel-chipped flint blade having one end tapering and usually curved like Type 2 ; the other end is blunt and often unworked and knobby. Varieties :—

Type 3 *a*. With concave cutting edge.

Type 3 *b*. With slightly concave or straight cutting edge.

Type 3 *c*. With convex cutting edge.

The author considers that the character of the butt-end shows that this type of sickle cannot have been hafted, and that therefore it must have been held in the hand. I feel very doubtful about this, and would prefer to associate this type with Types 1 *b* and 3 *c* in regard to hafting, and those cases in which the butt is actually thicker than the rest of the blade I should be inclined to associate with the flat bronze sickles with knobs (Type II below) in regard to method of hafting. Believing that Type 3 was unhafted, the author calls it the ' one-piece flint sickle '—a term which seems unsatisfactory, not only because its significance is ambiguous, being equally applicable to Type 1 *a*, but also because it is based on the assumed absence of a wooden haft, which is debatable.

This type evidently corresponds with the well-known British sickles of Grovehurst type, often miscalled ' crescentic '. Many of the latter show evidence of having been hafted in the Stenild manner (see Type 1 *b*). Dr Steensberg has not recognized this correspondence because he is under the mistaken impression that the British sickles are unworked blades, corresponding to his Type 1 *b*.

The occurrence of gloss on specimens of Type 3 is as follows :—

Type 3 *a* : of 23 specimens 9 are glossy ;
„ 3 *b* : „ 16 „ 6 „ „
„ 3 *c* : „ 3 „ 0 is „

Gloss appears to be much less frequent on the British series. No specimens of Type 3 have been dated by associated finds.

Bronze Sickles

In his typology of bronze sickles the author follows that of Hubert Schmidt, adding a fifth type that is peculiar to Denmark (fig. 2). As to dating, he follows H. C. Broholm's chronology which is shortly to be published. Types not found in Denmark are omitted here.

Type 1 *b*. Blade and tang form one curve ; ribs along back of blade curving smoothly downwards and continuing along both edges of tang. Of 3 specimens found in Denmark, two are datable to Late Bronze Age II (*c.* 600 B.C.). They are importations from the south.

Type II comprises the knobbed sickles, and is subdivided as follows :—

Type II *α*, in which the knob is long-oval and is placed transversely to the blade. Seven examples are dated to Early Bronze Age I (1400–1200 B.C.), and one to Early Bronze Age II (1200–800 B.C.). Thirteen others are undated.

Type II *β*, in which the knob is round and is situated in the angle between the butt and the back of the blade. Of this type 9 specimens are dated to Early Bronze Age I,

six to Early Bronze Age II, and 104 to Late Bronze Age I and II (800–600 B.C.). Fifty-three others are undated.

The round knob is thus a generally later type than the long-oval knob, though it originated just as early. There are no sickles with twin knobs, as in England.

TYPE V. Crescentic bronze sickle, corresponding to the concentric flint sickle. The edge is often serrated by means of a file, and the ends of the blade are sometimes 'reflex', i.e. curved backwards. This kind of sickle, like its flint counterpart, was

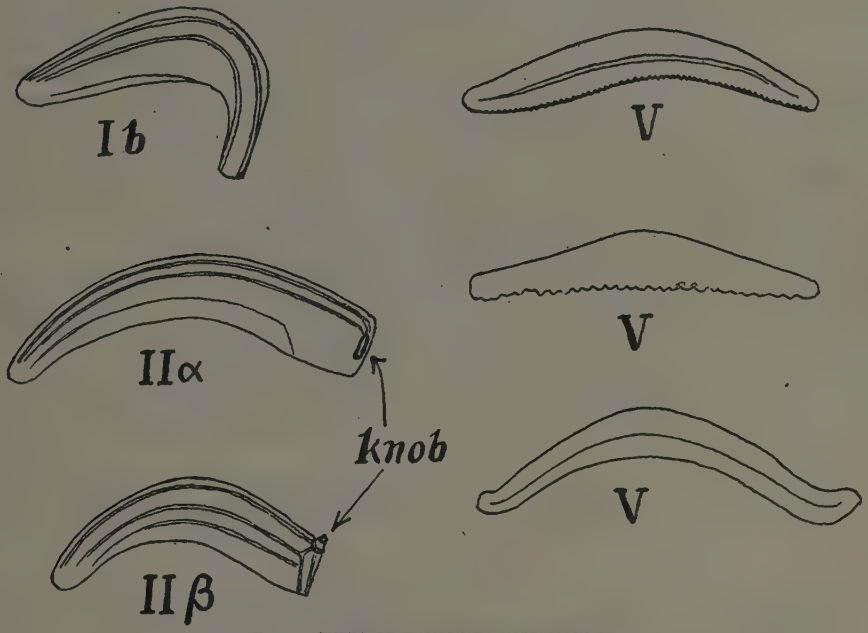


FIG. 2. TYPES OF DANISH BRONZE SICKLES

mounted in a groove along the concavity of a curved wooden handle, or perhaps in the concavity of a forked branch.

Of Type V twenty-six specimens are dated to Early Bronze Age I, and 39 are undated.

COMMENTS

It may be worth while at this point to summarize my own conclusions regarding flint and bronze sickles in Denmark, after careful consideration of the evidence as set forth by Dr Steensberg.

It would seem that, apart from the flint reaping-knives and the rare imported bronze sickles of Type I *b*, there have been within the Danish Stone and Bronze Ages two distinct methods of *hafting* sickles. The earlier (FIG. 3), which extended from the Dagger period to the Early Bronze Age (*c.* 1800–1200 B.C.), consisted in setting a crescentic blade, whether of flint or bronze, in a groove along the concave side of a curved wooden handle, as already mentioned. This, in my view, is a northern development

of the composite flint sickles, set in similar curved mounts, which are characteristic of the lands bordering the Mediterranean and as far north as the Alps (4).

The second method of hafting (FIG. 4) consisted in setting the butt of a straight or slightly curved blade into a hole at the end of a more or less straight handle, in such a way that the blade projects at right angles, as in the well-known specimen from Stenild. The handle has a projecting head which supports the back of the blade and prevents it from being torn out of its socket during use. This method, it seems, must have been used in the cases of flint Types 1 *b*, 2 *c* and 3, and of bronze Type II. Of these, flint Type 1 *b* would appear to be merely a more or less unworked ' poorer brother ' of Type 3, while Type 2 *c* is a crescentic flint which has later been re-shaped for hafting in the Stenild manner. Dr Steensberg has experimented with the hafting of bronze knobbed sickles, and after some failures he found that the Stenild method was the only one that stood up to the test of actual reaping (FIG. 4). The blade must have been fixed by inserting it right through a slot in the end of the handle, the function of the knob being to prevent it going too far. In this connexion we may note that a knobby-butted flint sickle of Type 3 could well have been mounted in exactly the same way. The chronological range of this type of hafting, then, coincides with nearly the full extent of the Bronze Age (say, 1600–600 B.C.), and thus overlaps the earlier method.

After all, the Stenild method of hafting can be regarded as a natural development from that of the crescentic blades, in the direction of simplification. In this view the blade, whether of flint or bronze, is firmly secured at the proximal end only, while the projecting head of the wooden handle is the morphological representative of the curved and grooved mount of the crescentic blade. It is noteworthy that while crescentic blades, whether of flint or bronze, are not found in Britain, sickles of both materials, demanding hafting in the Stenild manner, are met with, though not as commonly as in Denmark.

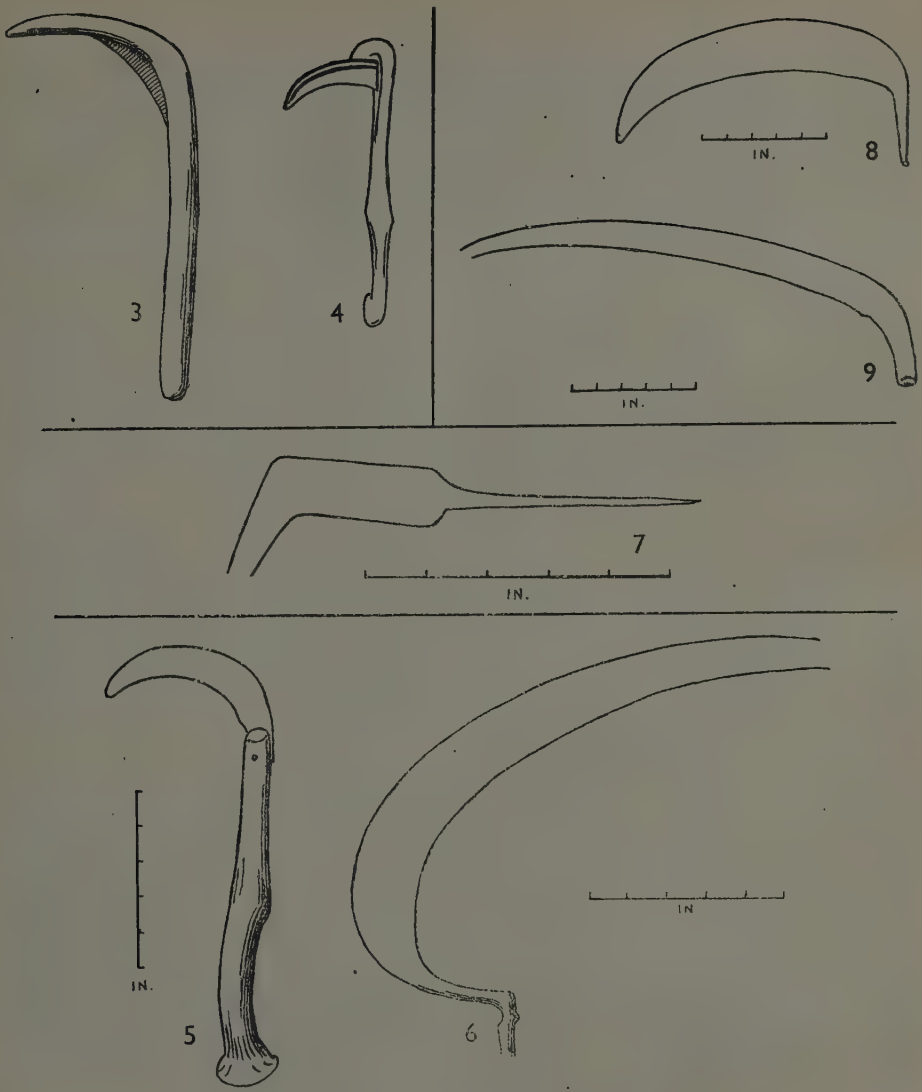
Bronze Type 1 *b*, which is dated to the Late Bronze Age and is an import from the South, appears to be a development from the crescentic blade set in its curved haft.

Iron Sickles

Iron sickles fall into two main groups, the *angular*, and the *balanced* (FIGS. 5, 6). In the former the weight of the blade lies entirely to the left of the haft while the instrument is in use, as a consequence of which there is a constant tendency for the point to turn downwards towards the ground. In order to counter this the haft is so shaped that it will not revolve easily in the hand ; this is usually achieved by making the nearer end hook round the little finger (FIG. 5). The double advantage of the balanced sickle lies in the complete elimination of this tendency for the blade to turn downwards, thus permitting the use of a much longer blade. This is achieved by curving the blade and its tang into the form of a query-mark, so that the axis of the tang and haft would, if produced, pass through the centre of gravity of the blade (FIG. 6).

In the Iron Age only *angular* sickles are found. These have been evolved from bronze sickles of Types 1 *b*, II and IV (the latter not found in Denmark). The blade may be more or less curved, but at the butt it usually passes into a flat tang running along the underside of the handle, where it is secured with nails and by driving the bent-up point of the tang into the handle ; or else it may be secured by nails alone. The bent-up point of the tang, which survives in our modern scythe, is regarded as a development from the knob of the bronze knobbed sickle (Type II).

[4] *Proc. Preh. Soc.*, 1938, IV, 31.



FIGS. 3, 4. METHODS OF HAFTING FLINT AND BRONZE SICKLES

3. Crescentic blade of Flint or Bronze in grooved haft
4. Knobbed bronze sickle mounted in the Stenild manner

FIGS. 5, 6. ANGULAR AND BALANCED SICKLES

5. Angular iron sickle with original wooden handle from Vimose (Roman Iron Age)
6. Balanced sickle from Randers (medieval)

FIG. 7. MEDIEVAL LEAF-KNIFE

FIGS. 8, 9. BLADES OF SHORT-HANDLED SCYTHES

8. Roman Iron Age (Illemose)
9. Viking period (Skovlunde Mark)

The usual method of sharpening sickles and scythes in the Iron Age was by means of a hammer and small anvil. This is a legacy from the Bronze Age, for bronze must be sharpened by the hammer. It is all the more interesting to note that the hammer-sharpening of scythes survives in Denmark and Germany to this day. But in the Viking Age, especially in Norway, a new method of sharpening appears, viz., serration by means of a file. As in the serrated bronze sickles (Type v) the teeth of the serrations point inwards towards the handle, showing that in cutting corn with these sickles the stroke was from left to right, and not from right to left as with the modern swap-hook and scythe.

The angular sickle has lingered down to recent times in parts of Norway, Sweden and Finland.

The *balanced* sickle first appears in the La Tène period, both at La Tène itself and in Transylvania. It is common on Roman sites, and has been found in various sites of later occupation in Europe. In Denmark it is not till the Middle Ages, i.e. after the Viking period, that the balanced sickle completely ousts the angular variety. The usual method of hafting is a spike tang driven into the end of a turned wooden handle, a method of fastening which is made possible by the balancing of the blade.

Leaf-Knives

Whereas in the Stone and Bronze Ages the same implement may have been used for a variety of more or less related purposes, in the Iron Age we begin to find more specialization of tools, due largely to the abundance of iron and the ease with which it could be worked. Thus we find the sickle being differentiated into four tools : the sickle itself for cutting grain, the scythe for cutting hay, the leaf-knife for gathering leaves for fodder, and the pruning-hook, primarily for viticulture. The appearance of scythes and leaf-knives at this point may also indicate the increased importance of winter fodder at a time when the climate underwent marked deterioration.

A few bronze leaf-knives, based on the bronze sickle Type 1 *b*, and belonging to the Late Bronze Age, have been found in south and central Europe.

The leaf-knife may be regarded as a sickle in which the blade was set very nearly in the axis of the handle, instead of at right angles to it, as in the angular sickle. The tip of the blade is usually curved into a slight hook, and it was hafted by means of a spike-tang, as our modern bill-hooks are (fig. 7). In fact, the author evidently regards bill-hooks and pruning-hooks as varieties of the leaf-knife. In the Danish island of Bornholm there is a tradition that leaves were once harvested for fodder, and there is an old superstition elsewhere in Denmark that the maids must not fetch leaves for the cattle on St. Bartholomew's day. Apart from these scraps of tradition the harvesting of leaves has long been not only extinct but forgotten, but bill-hooks are still used for trimming hedges and chopping sticks, and one cannot always differentiate between the leaf-knife proper and the bill-hook.

Scythes

Iron scythes are of two kinds, the short-handled and the long-handled.

The *short-handled* scythe first appears in the Roman Iron Age in Denmark (fig. 8). It is in part a derivative from the angular sickle, having a rather short and broad crescentic blade (usually 12 to 14 ins.) set at a somewhat obtuse angle on a tang which terminates in an up-turned point for driving into the underside of the handle. The joint was additionally secured by an iron ring, and presumably a wedge of wood, as in the modern Norwegian examples. This type of fastening the blade to the haft is clearly derived

from that of the angular sickle, with this difference, that the nail has been replaced by the ring and wedge, to facilitate setting the blade at the required angle. It is, too, a method that has survived practically unchanged in our large modern scythes.

In the Viking period the hafting angle becomes more obtuse, and the blade becomes longer, straighter and more slender, but with the point curving inwards (FIGS. 9, 11). The blade is often about 18 ins. long, and its width is most commonly from 1 to 1½ inches.

The typical La Tène scythe—the *crescentic* scythe, not found in Denmark—is derived from the bronze sickle, Type 1 *b*. It may well have had some influence on the form of the earliest Danish scythes (Roman Iron Age).

The handles of three short-handled scythes found at La Tène have been preserved intact; their length is from 30 to 34 inches, and they must have been wielded with both hands, because it is only so that they can be held in balance. While the short-handled scythe appears to have become extinct in Denmark after the 12th century, it is still in common use from Norway to North Russia. The modern Norwegian examples resemble those of the Viking period, having the blade about 18 inches long, made of soft iron, but the hafting angle is not so obtuse, the tang forming something like a right angle with the blade (FIG. 10). This change in the hafting angle is a result of the balancing of the blade by means of an angular handle. This means that the wooden handle is bent at an angle near the middle, in such a way that the axis of the proximal half passes through the centre of gravity of the whole implement. Thus balanced, this useful little scythe can be wielded with one hand after some practice.

With a scythe the direction of stroke is different from that of an angular sickle. The latter is used with a stroke from left to right, as shown, among other things, by the direction of the teeth on serrated sickles. The scythe, however, must be used from right to left, and it was to facilitate this shearing movement that, previous to the balancing of the blade by means of an angular handle, the hafting-angle was so obtuse.

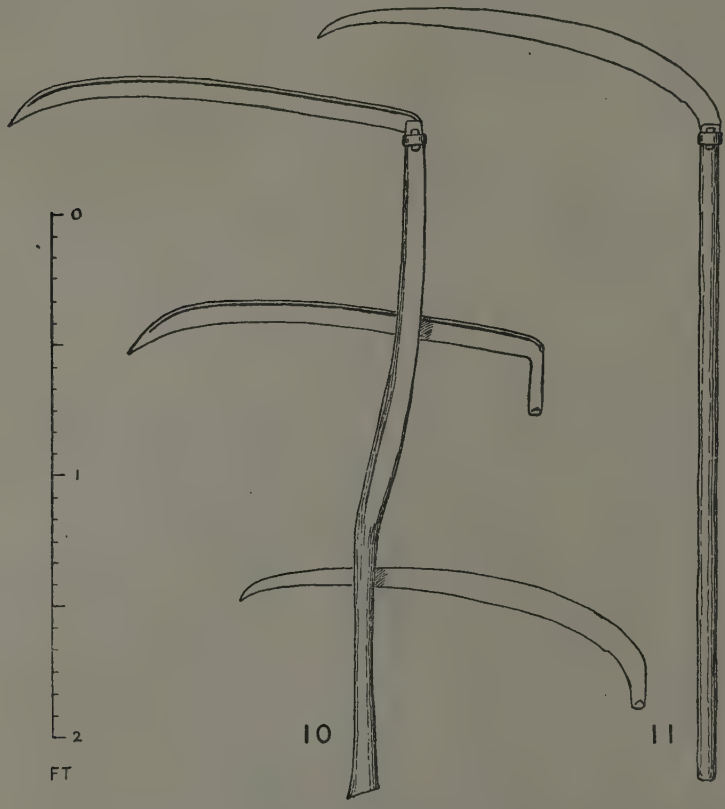
In certain countries, e.g. Holland and the Baltic States, the short scythe is used for grain-reaping—evidently a secondary use whereby the scythe has usurped the place of the sickle. The author discusses the possible causes of this change, suggesting that when the short scythe became balanced, as described above, so that it could be wielded with one hand, the left hand was free to use a mowing-crook such as one still sees used in connexion with the swap-hook in this country. If I may venture an alternative suggestion it is this. In reaping grain with a sickle the general habit of antiquity was to cut the stalks rather high up, either about the middle or below the ears; this entailed a second cutting near the ground if the straw was to be made use of. It ought to have become fairly obvious to the ancient farmers that standing corn could be cut near the ground with a scythe, just as hay is, with the advantages of having to cut it only once, of cutting it more rapidly, and of avoiding the continual stooping involved when the sickle is used near the ground.

The *long-handled* scythe has a different origin, and must be traced back to the balanced scythe that is found in Celtic times in Transylvania and Austria. This in turn is said to have been derived from the bronze hooked sickle (Type III) which is found in those parts. Under the Romans a long-handled scythe was developed, which spread to Germany and Britain. The length of the chord of the blade in these implements measures from 32 to 43 inches, and the hafting angle is remarkably acute. As the blade no longer has the rearward curve of the balanced scythe (which was like a very large balanced sickle), the acute hafting angle presumably indicates that the balancing of the implement was now effected by means of a curved sneath, as in the modern scythe.

In Denmark the long-handled scythe does not appear until the Middle Ages. The

evidence concerning the medieval long-handled scythe is principally derived from contemporary illustrations in manuscripts or in mural decorations in churches.

In Italy in the early Middle Ages we find scythes depicted with curved sneaths, arm extensions and single handles. In France straight sneaths, arm extensions and either one or two handles are shown. In the type with two handles the blade is socketed.



FIGS. 10, 11. SHORT-HANDLED SCYTHES

Note the up-turned point of the tang of each blade, also the method of attachment by collar and wedge

10. Modern Norwegian, with angular handle for balance

11. Viking period (Danish), with modern straight handle based on original Swiss specimens

N.B.—The sneath of the long-handled scythe is about twice the length of the handles shown above.

Something similar to the one-handled type survives in use to this day. With a straight sneath the handle must be relied on to counteract the imbalance of the blade. Both these types occur also in medieval England, and are said to survive to the present day.

SCYTHE *versus* SICKLE

After further discussion of the various types of long-handled scythe that have been recorded in Europe the author proceeds to examine the way in which the scythe has

gradually ousted the sickle as a harvesting implement for grain. For this purpose he has studied over 2000 Danish farm inventories covering the century 1672 to 1775. As a result he finds that whereas the sickle was still in common use at the beginning of that period, its use was confined to parts of West Jutland at the end of it. By 1833 the sickle was only used in one county (Ringkøbing). In Norway the sickle was still found in use in 1872, in Sweden in 1899, and in Finland at the present day. It appears that although the scythe does the work in about one-fifth of the time taken by the sickle, yet where the labour of women and children was plentiful farmers often preferred to use the sickle. This was especially true in regard to the harvesting of rye, the seeds of which are more easily knocked out by the scythe than by the sickle.

In England the sickle was still preferred to the scythe in 1855, when Henry Stephens gives the following comparative figures: in the course of a ten-hour working day 2.3 acres of wheat per man can be reaped with a scythe as compared with 1.1 acre with the smooth-edged sickle or 1.0 acre with the serrated sickle. For barley and oats the corresponding figures are 4.0, 2.2 and 2.0 acres respectively. In the Hebrides corn was reaped exclusively with the sickle in 1811, and I may add that the sickle is still so used there occasionally (5). The author discusses the many factors that have governed the change-over from the sickle to the scythe, and he points out that these are very similar to the factors that have led to the change from the scythe to the reaping-machine in our own day.

Primitive Harvesting

At the dawn of agriculture was the corn cut or merely uprooted? The author discusses this question at some length and concludes that, although uprooting is known to have been practised sporadically among some historical cultures, yet it cannot be assumed to have been the normal forerunner of cutting. A great deal depends on botanical questions such as the relative toughness of the straw and the fragility of the rachis, and also on such practical matters as whether the straw was valued as well as the grain. The earliest known methods all involved the severing of the ears from the straw.

The earliest known harvesting implements are then reviewed, beginning with the composite flint 'reaping knives' found by Professor Garrod in the caves of Mount Carmel. These famous mesolithic pre-agricultural harvesting tools (if we may so term them), are almost sufficient in themselves to refute the uprooting theory.

Experiments with Primitive Sickles

Not the least valuable part of Dr Steensberg's book is the section describing the experiments he has carried out with various kinds of sickles, in order to learn something of their relative efficiency. In order to carry out the test under uniform conditions each implement was used to reap a plot of 50 sq. metres in a field of barley containing nearly 18 per cent of oats. Straw counts were kept in order to ensure that the work done by each implement was comparable, and the figures obtained have been adjusted with this end in view. Modern handles were provided for the ancient sickles.

The following list gives the number of minutes taken in reaping a plot of 50 sq. metres with the particular implement indicated.

Modern Slovakian sickle	31 min.
Modern Galician serrated sickle	30 „
Bronze knob-sickle (copy) (Type II)	64 „
	60 „

[5] I have a photograph of a woman reaping oats with a sickle near Stornoway, taken in 1937.

Crescentic flint sickle (Type 2)	59 min. 68 „
Crescentic serrated bronze sickle (Type v)	65 „ 66 „
Crescentic serrated flint ('saw')	73 „
Stenild sickle (copy with ancient flake, Type 1 *b*) ..	76 „
Stenild sickle (copy with sharp flake)	101 „
Unhafted flint sickle, curved (Type 3 *a*)	90 „
Unhafted flint sickle, straight (Type 3 *b*) (only 47.5 sq. m.)	72 „
Short-handled Roman scythe (copy) (only 49 sq. m.) ..	30 „
Short-handled Viking scythe (copy)	17 „

Where two figures are given in the above list two experiments were performed with the same or similar implements. The greatly increased reaping time for the copy of the Stenild sickle with the sharp flake was due to that experiment having been performed by a different person, not so accustomed to wielding these strange implements.

It will thus be seen that most of the ancient sickles took approximately twice as long as the modern iron sickles. Those that took longer were the flint 'saw', the copies of the Stenild sickle, and the unhafted flint sickles, one of which took three times as long. On the other hand the Roman scythe equalled the time of the modern sickles, while the Viking scythe worked nearly twice as fast.

Even more important than speed, in the author's estimation, is the percentage of straws uprooted by each implement. The worst offenders in this respect were the flint 'saw' and the copy of the Stenild sickle with the ancient (and therefore blunt) flake. The serrations of the former were so coarse that they caught and pulled on the straws. For these reasons the author is disposed to regard the 'saw', as having been used for some other related purpose such a leaf-knife, and he thinks that the Stenild sickle, being in his view less efficient than the other sickles, may have been used as a weeding hook. I do not feel quite satisfied with this last opinion in view of the important place the Stenild method of hafting must have taken during the Bronze Age, as indicated earlier in this review.

Preliminary experiments had shown that the straight and convex-edged crescentic flints were so inefficient when used for reaping that they could scarcely have been used as sickles.

Conclusions

In the above outline I have had to be selective, for the book is long, and so packed with detailed information that one may suspect that the author himself cannot always 'see the wood for the trees'. The following is an abridgment of his conclusions.

The origin and evolution of implement forms are not fortuitous but result from *adaptation* to (1) natural conditions, and (2) the prevailing form of culture. To understand what underlies adaptation the function of the implement must be examined and compared with the function of similar modern implements. Next the geographical and chronological distribution of the *form* of the implement must be studied, and then its *quantity*, which requires a complex of common culture elements as proof of a cultural continuum.

The present investigation has shown how important it is to determine the methods of hafting implements. This has a bearing on their use, and is therefore a suitable indicator for type classification.

Notes and News

HAD THE EGYPTIANS AN ALPHABET?

I read Dr Diringer's recent article on 'The Origins of the Alphabet' in the June number of ANTIQUITY with the greatest interest. But in one respect I find myself in disagreement with him; to my mind it is quite clear that the Egyptians did possess an alphabet and on occasion used it as such. I have already touched briefly on this topic in my review of Sethe's 'Vom Bilde zum Buchstaben' in *Jour. Egyptian Archaeology*, XXVII, p. 169 f., but in the circumstances it seems desirable to take it up again. The basic facts are :—(1) among the many ideographic and phonetic signs employed in writing Ancient Egyptian there exist 24 uniconsonantal signs which cover the whole range of the consonants employed in Egyptian speech (the vowels were not written); (2) contrary to Dr Diringer's statement on p. 77, very many common words were regularly spelt out letter by letter with the above-mentioned uniliteral signs in exactly the same manner as we spell out our words, without any phonetic or ideographic additions, while a great many other words were similarly spelt out alphabetically, but ended with an ideogram which served both as 'determinative' of the sense and as word-separator,[1] but which played no part in the 'spelling'; (3) even where custom decreed that a word should be written with bi- or triliteral phonograms with or without the addition of single letters (the so-called 'phonetic complement'), the Egyptian could when he chose break the word down into its simplest elements and spell it purely alphabetically, cf. the case of the verb for 'mix' quoted in the above-mentioned review; (4) single letters were regularly employed to spell out grammatical endings. When it is further pointed out that a few late inscriptions are actually written alphabetically throughout, it is impossible to deny that the Egyptians not only possessed an alphabet but knew how to use it. Demotic, to which Dr Diringer briefly refers, is but a very cursive derivative of hieratic, itself but a cursive script adapted from hieroglyphic, and is in no way a 'simplification' of hieratic; while the reason that, when the Egyptians finally decided to adopt an alphabetic script for daily use, they chose the Greek Alphabet (supplemented by a few signs taken from demotic) was undoubtedly the fact that at that late date most educated persons had a smattering of Greek, whereas the hieroglyphic script was dead except for the limited circle of the priesthood.

The stumbling-block in the way of Dr Diringer and the other scholars who share his view is the fact that the Egyptians, having acquired an alphabet, did not discard the unnecessary phonetic lumber with which their script was burdened. Here, however, they fail to make allowance for the innate conservatism of the Egyptians, who, when they made a new advance in thought, refused to discard the now out-moded mental concepts which tradition had sanctified, and in fact were quite capable of holding contradictory ideas at one and the same time. This traditionalist tendency receives visual expression in the writing, which combined picture-signs (ideograms), multiliteral phonograms and single letters into a complicated script which thus crystallized all the stages of the development of writing. The Egyptians were capable of simplifying this into a purely alphabetic script, though with a few late exceptions they did not choose to do so; indeed the retention of the phonograms and determinatives gives individual words a characteristic

[1] See A. H. Gardiner, *Speech and Language*, p. 122.

appearance which at times is a real aid to interpretation. To deny the existence of a true Egyptian alphabet on the ground of the coexistence of other classes of signs is to fly in the face of the evidence ; if a group of uniliteral signs adequate to spell out any word in the language letter by letter does not constitute an alphabet, then the term is indeed difficult to define. The fact that the Egyptian alphabet was obtained as it were accidentally,[2] and not by the invention of a gifted individual, does not make it any the less an alphabet.

As regards the relation of the Sinai inscriptions to the Phoenician and other early Semitic scripts, space does not permit lengthy discussion here ; Dr Gardiner restates his case and discusses his critics' objections in *The Legacy of Egypt*, pp. 55 ff. It must suffice now to point out that Dr Diringer, who accepts Gardiner's reading of the name *Ba'alat*, does not appear to realize what that acceptance involves. This reading was obtained on the basis that the Sinai letters were the pictorial equivalents of the Semitic and Greek letter-names, i.e. that the Semitic letter *aleph* (=' ox ') was written in Sinai with an ox-head,[3] *beth* (= ' house ') with the diagram of a house, *'ayin* (=' eye ') with the drawing of an eye, and so forth. If, therefore, the reading *Ba'alat* be accepted, it follows of necessity that the Sinai script must be the ancestor of the Phoenician and later alphabets, since the letter-names are clearly the same. To deny the descent of the Phoenician alphabet from that of the Sinai inscriptions involves also the denial of the reading *Ba'alat*, and hence of the validity of the method by which that reading was obtained, though none of the sceptics have proposed a plausible alternative. Unless a fundamental fallacy in Gardiner's reasoning can be demonstrated, there seems no choice but to accept his reading *Ba'alat*, together with all the consequences which flow therefrom.

R. O. FAULKNER.

REJOINDER

I am very grateful to my friend Faulkner for his remarks. It must be very useful for the reader of this excellent Review to know the important scientific problems discussed by people of different opinions. I have tried to be objective as far as possible, but only so far. Mr Faulkner, as a true Egyptologist, and as a collaborator with Dr Gardiner, is certainly the right man to defend the Egyptian theory of the origin of the Alphabet, and particularly the Sinaitic theory.

I shall not discuss here again the whole problem, which I believe to have fully examined in my book on the *Alphabet in the History of Civilisation*, and in my articles and lectures since 1935, when I first expressed my opinion on this subject at the International Congress of Orientalists in Rome. However, I will examine briefly Mr Faulkner's doubts.

(1) Had the Egyptians an alphabet ? is the question asked by Mr Faulkner. To his affirmation, I must say ' no '. Thus the discrepancy seems enormous ; but it is not so, at least not directly. Mr Faulkner attempts to prove that he is right. I accept his proofs of evidence which I had mentioned already in my above recorded book. It is not in this respect that we disagree, but as to the exact meaning of the expression ' Alphabet.' According to my opinion (see ANTIQUITY, June 1943, p. 77), ' in a true alphabet each sign generally denotes one sound only, and each sound is represented by a single, constant symbol, while in the Egyptian scripts there existed different signs for the same sound ; thus the same sound could be written in many ways '.

[2] cf. Gardiner's remarks in *The Legacy of Egypt* (ed. S. R. K. Glanville), p. 64.

[3] This particular letter does not appear in the name *Ba'alat*.

(2) Even if we make all possible allowances for the conservatism of the Egyptians, we cannot still understand why they could not use their own alphabet instead of the demotic writing, which certainly had no such tradition to bolster it up, as the hieroglyphic and in much lesser degree, the hieratic scripts.

(3) As regards the relation of the Sinai inscriptions to the alphabet my opinion is still far from that of Mr Faulkner. Although the Palaeo-Sinaitic inscriptions have been known since Sir Flinders Petrie's discovery in 1904–5, and the Sinaitic theory of the origin of the alphabet was propounded by Dr Gardiner in 1916; and notwithstanding that many eminent scholars have dealt with this problem and with that of the decipherment of the Palaeo-Sinaitic writing; in spite finally of the discovery of, and the research in the Canaanite inscriptions, which according to some scholars 'constitute an important "missing link" in the history of our own alphabet, representing the long-sought intermediate stage between the Sinaitic and the earliest known Phoenician forms';—we are nowadays exactly in the situation very well summarized by Dr Gardiner himself 27 years ago. 'Unfortunately, however, I have no suggestions for the reading of any other word, so that the decipherment of the name Ba'alat must remain, so far as I am concerned, an unverifiable hypothesis'. (*J.E.A.* III, January 1916, p. 15). How very different has been the history of the decipherment of the Ugarit alphabet! What is the reason for this difference? According to my opinion it lies mainly in the fact that the Palaeo-Sinaitic inscriptions do not yet offer sufficient material for their decipherment, as little as they can help us to solve the problem of the origins of the alphabet.

DAVID DIRINGER.

WAR AND ANCIENT MONUMENTS

The following statement is printed from the written answer given by the Secretary of State for War in reply to an enquiry in the House of Commons as to measures taken for the preservation of ancient monuments in Cyrenaica and Tripolitania.

'When the British Forces advanced into Libya in the autumn of 1942 immediate steps were taken for the preservation of any archaeological monuments which might come into our possession during the course of the occupation. In the case of Cyrenaica, similar steps had been taken during the two previous occupations of the territory and despite Axis allegations to the contrary it is believed no damage of any importance was done to the ruins at Cyrene, Appollonia, Ptolemaide or Tocra.

As was anticipated, the Staff of Guardians previously employed by the Italian administration was found carrying on its work at all the important ancient sites and these guardians were retained in the employment of the administration and paid their salaries. This arrangement continues. In the case of Cyrenaica, only Arab guardians remained at their posts and in order to strengthen their powers in dealing with any troops who might cause damage to the ruins a British non-commissioned officer of low medical category has been posted in charge of the guardians.

The museum at Cyrene was practically cleared of all exhibits when the Axis forces first retreated from Cyrenaica in January 1941, but a few large pieces remain. On this occasion the museum was securely closed as soon as possible after the building came into our hands. After our withdrawal from Cyrenaica on this occasion, the Italians prepared a propaganda pamphlet in which they purported to enumerate the acts of vandalism perpetrated by our troops during our three months' occupation of the territory. Among other things they alleged that the Australian troops did very considerable damage to the exhibits in the Cyrene museum and they published a photograph showing

a large quantity of broken vessels and damaged statues said to have been taken in the Cyrene museum. Investigations made during this last occupation of Cyrenaica proved conclusively that this photograph was not taken in the Cyrene museum at all. It was in fact a photograph of a small shed adjacent to the museum in which the Italian archaeologists had collected a large number of broken vessels and damaged statues and were in the process of piecing them together. The photograph published by the Italians was therefore grossly misleading and in no way supported the charges made against our troops.

Soon after the occupation of Cyrenaica arrangements were made for the Director-General of the Alexandria Greco-Roman Museum to visit Cyrenaica and to make a full report regarding the condition of the ruins, the adequacy of the steps taken for their preservation and the action advised for conserving the ruins on a care and maintenance basis. A number of photographs has been taken in order that there may be a permanent record of the state of the ruins at the time of our occupation. The Deputy Chief Civil Affairs Officer, Cyrenaica, and the District Commander are both fully alive to the importance of safeguarding the monuments which are in their custody.

In the case of Tripolitania, similar steps were taken as in Cyrenaica to retain in the employ of the military administration the guardians, both Arab and Italian, who were responsible under the Italian administration for safeguarding the sites. In addition the skilled Italian archaeologists who remained in the country have been kept on the pay-roll of the British military administration. The large and important museum at Sabratha is completely intact and is being very carefully guarded. Steps have also been taken to safeguard the ruins at this site. The advice of several experienced archaeologists has been obtained and an archaeologist has been seconded to the British military administration at Tripolitania in order further to advise the Deputy Chief Civil Affairs Officer on the measures which should be taken to safeguard all the ruins in the territories'.

WINGHAM VILLA, KENT

I would venture to bring to notice certain statements in George Dowker's second report on the excavation of Wingham Villa, in *Archaeologia Cantiana*, 1883, xv, 351, which seem to me to have escaped notice in recent years and to be of sufficient importance to justify re-excavation. Dowker states that he discovered remains of a post-Roman occupation of this Villa.

Of course it is possible that Dowker mistook for post-Roman remains late-Roman repairs which may have been executed after, say, the ravages of 368. If however Dowker was correct, it would mean that here in a part of East Kent where continuous occupation of a Roman site is, if anywhere, to be expected, there was a Villa occupied after the Roman period. The relevant passages in the report are given.

'Northward and at right angles with the northwest line of the wall of the hypocaust, we have traced a block of buildings in great part destroyed, consisting partly of Roman, and partly of other badly-built flint walls laid on Roman material, but of inferior workmanship. I have prepared a plan of them, but they were in places so disconnected, so wanting in the strength and solidity of the former buildings, that I am compelled to believe they were not of Roman workmanship. On digging deeper I found the remains of a Roman wall, with its characteristic good mortar lying beneath, though sadly broken up in places. This wall extended 14 feet northward, in continuation of the extreme western wall of the hypocaust; it then turned away to the east, at right angles, for about 12 feet, where its remains were buried beneath later walls, partially destroyed. Another wall,

set off at right angles with the hypocaust wall, also partially destroyed, was met with extending 12 feet north, and having a turn 5 feet east. The end of this had a narrow wall mostly formed of Roman roofing-tiles built on to it '.

After describing certain Roman objects discovered, Dowker says :—

' Scattered over these, in the débris of the foundations, were found coarse black pottery apparently belonging to culinary vessels ; Upchurch ware in some abundance ; bones in great abundance of the ox, pig, and deer ; mill-stones and fragments of querns (one of puddingstone and one of lava) ; a Roman coin of Antoninus Pius, with a hole bored through it, as though used for a charm. These, with the destruction of the earlier foundations, the apparent use made of the tessellated rooms for mill purposes, the abundance of culinary vessels, and the wide-spread extent of the débris, show that its site was occupied by a semi-barbarous people, who had used up the material for other purposes, utilizing only what suited them for the farm or the mill. The evidences of this are found in examination of the hypocaust buildings, the suspended floors of which (having fallen in upon the ashes filling the flues beneath) were afterwards built up with ruder material and utilized for other purposes '.

Dowker then mentions further excavations and says that he found the floor of a room paved with red tesserae of Roman tiles which were merely resting on the earth. ' From this ', he says, ' we uncovered walls 40 feet in length, and others parallel 25 feet distant with one enclosed rectangular space 25 feet square at right angles to the latter and more to the east. These walls were in most cases less than one foot from the surface of the field ; they were composed of flints and friable mortar, that yielded readily to the pick ; they were not quite regular, and not quite rectangular in places, 2 to 3 feet wide. They rested at places on Roman tiles and débris of Roman workmanship '. He points out that very few relics of modern and medieval date were found and says that these walls were about 100 yards southeast from the bath. ' What connexion had they with it ? Were they part of the Roman Villa ? Were they of later Saxon date ? It is difficult to give a decided answer to these interesting questions. I have most carefully looked for any clue to guide me. The direction of the walls crosses the wire fence (which runs nearly due east and west) at an angle of about 35 degrees. The wall of the bath cuts the wire fence in the same direction, but not at the same angle. I think these facts point to the conclusion that some Roman buildings existed here, connected, it may be, with the hypocaust buildings. These had been burned down and overthrown, and subsequently rebuilt for agricultural purposes by Saxons, making use of the Roman material '.

These are, I think, the relevant passages, and in view of the fact that a Pagan cemetery was found on the neighbouring farm, it is possible that Dowker's opinion that the later buildings were early Jutish is correct. Though, as I say, they may have been late Roman. I do not know if Dowker's plans are still in existence. It is to be hoped that they are, for his statement that some of these walls ' yielded readily to the pick ' sounds sinister.

I would however strongly urge that an attempt should be made to re-excavate this site and also the Villa at the neighbouring village of Ickham which by 1883 had not been fully excavated, and, as far as I know, has not been excavated since. There is another Pagan cemetery not far from there and it may be that that Villa too was occupied in post-conquest times.

It may seem odd that this paper of Dowker's should have escaped notice ; but I think it must have done as Haverfield's assertion that no Saxon dwelling has ever been found on the site of a Roman Villa, is repeated by Professor Collingwood in his *Roman Britain*, published as recently as 1935, and he repeats the assertion with particular reference to Kent. Possibly he was thinking of the many Villas in West Kent.

Apart from two residences—one at Birchington and the other at Folkestone, the latter of which may not have been a Villa in the economic sense at all—only two Villas have been found in East Kent, and it may well be that these two Villas were occupied after the conquest, unlike the Villas in the Medway area which in so many respects differs archaeologically from East Kent, notably in the complete absence of Kendrick's type 'A' jewellery.

A. G. WELLS.

TRUMWINE'S DIOCESE

In 681 'the province of the Picts which at that time was subject to the rule of the English' (Bede, *Ecclesiastical History*, IV, 12) was constituted a diocese, to which Trumwine was appointed as bishop. He administered his bishopric from Abercorn, in Anglian territory but on the south coast of the Firth of Forth, which divided the lands of the Picts from those of the English (Bede, IV, 26). This Pictish 'province' under Northumbrian influence must, therefore, have been either north of Abercorn (i.e. in Fife), or west of Abercorn (i.e. in West Lothian).[1]

It may be suggested, however, that when Bede speaks of a 'province of the Picts' he is using that term to refer to one of the seven provinces, into which Pictland was divided, as Skene believed, by 710, when Ceolfrid's mission visited king Naitan. Each province had its own dynasty, now one royal line and now another, providing an overlord for the whole confederation. Thus Brude mac Bile, Ecgfrith's opponent in 685, is called by Tigernach 'king of Fortriu' ('rege Fortrenn'); another province was ruled by Talorgan mac Drostan, 'rex Athfotla',[2] who died in 739. Adamnan, a contemporary of Bede, refers on several occasions to 'the province of the Picts' under the sovereignty of Brude mac Maelcon. Elsewhere Bede himself speaks of 'the provinces of the Northern Picts' (III, 4), and of 'all the provinces of the Picts' (V, 21), while he describes as a 'province of the Picts' the kingdom of Brude mac Bile, which Ecgfrith invaded. If, then, Bede's statement is to be interpreted in the sense that one of these seven provinces formed the diocese of Trumwine, it can only have been Fife (*Fib*).

If Fife was Trumwine's diocese, it follows that this province was included in Oswiu's conquest of 'the greater part of the Picts' (Bede, III, 24) about 658. Thus the territory of the 'Niduari' Picts ('Niuduera regio'), in Fife or its neighbourhood, which St. Cuthbert visited, may well have been part of the Northumbrian dominions (ANTIQUITY, XIV, 289). Nechtanesmere gave back to the Picts 'their own land which had been held by the English' (Bede, IV, 26); and Trumwine, bereft of his diocese, left Abercorn. If, however, Fife had been that diocese, it would be a reasonable explanation of Eadberct's march in 756 to 'Niwanbirig' (Newburgh in Fife), that he quarrelled with his ally, Angus mac Fergus, and tried to recover the province which had been ruled by his predecessors for 27 years. The Northumbrian connexion, at any rate, was strong enough for Angus mac Fergus (731–761) to reconstitute under Northumbrian influence the monastery of St. Regulus at Kilrymont (St. Andrews). Here relics of St. Andrew were brought, probably from Hexham (ANTIQUITY, XVI, 9). The place-name 'Niwanbirig' is itself evidence of the presence of English-speaking settlers in Fife; and such settlement may not have been confined to the south shore of the Firth of Tay if Fotheringham in Angus (1424 Fordryngham, Fodiheryngham, 1492 Forthringham) contains an O.E. habitation suffix in *-ingaham*. Even if this is only a termination in *-ham*, the name must still be relatively early. The continuation of the cultural influence of Northumbria

[1] Angus Macdonald, *The Place-Names of West Lothian*, XVII

[2] W. F. Skene, *Celtic Scotland*, I, 280, 266n, 281n.

into the 9th century is shown by cross-slabs or cross-shafts with the typical Northumbrian motive of the vine-scroll, which in Fife occur at Mugdrum and St. Andrews (ANTIQUITY, XI, 471). W. R. KERMACK.

THE STUDY OF ANTHROPOLOGY

By permission of the EDITOR of THE SCOTSMAN we are able to print an article, by Professor V. Gordon Childe, contributed to that Journal on the occasion of the centenary of the Anthropological Institute of Great Britain and Ireland.

Just a century ago twenty-three enthusiastic students of humanity, meeting in a London room, founded the Ethnological Society and incidentally elected a Scotsman, Vice-Admiral Sir Charles Malcolm, its first president. From this meeting was born the Anthropological Institute of Great Britain and Ireland as it has been correctly called since 1871, when the Ethnological Society of London (then presided over by T. H. Huxley) fused with a younger rival and ally, the Anthropological Society, founded in 1863. The centenary of the Institute thus formed, to be celebrated in London tomorrow (30 October 1943), marks at the same time the coming of age of the Science of Man—the objective, systematic and co-operative study ' of the Human Race in all its varieties and in all phases of its history and progress ' as the sub-title of the parent Society put it.

The disciplines the young society set out to pursue—or rather to create—are now accepted as sciences worthy to be taught in Britain's greater Universities and capable of providing practical guidance to colonial administrators and Departments of State. But in 1843 there was no scientific archaeology or ethnography. The very principles of classification that form the basis of any science had still to be formulated ; appropriate methods of observation had to be discovered. The nucleus of the first Ethnological Society was composed of members of the Aborigines Protection Society, members, however, who believed that the most practical way of advancing the interests both of ' natives ' and humanity as a whole might not after all be to agitate for the conferment upon Hottentots and Fijians of trousers, parliamentary democracy, the factory system or similar benefits, but first to ascertain the truth about their political institutions and economic organisations, their crafts and beliefs, their history and our history too. The century has justified this belief.

More slowly colonial administrators and at last, the Colonial Office itself, took in that native political institutions, legal systems, rules of inheritance, and proprietary rights were so different from Anglo-Saxon and Roman that even the best intentioned applications of the latter might be very unjust and even disrupt society itself. A University course in social anthropology is now obligatory on all destined for administrative posts in the colonial Empire. The public and official recognition of the interest and utility of anthropology is no doubt due on the one hand to the often painful results of experience, on the other to the lectures and books of such genial exponents of the subject as Tylor, Frazer, Keith and Malinowski. But the material these expounded, the methods of approach taught to missionaries and colonial officials, had been largely gathered and systematized, developed, and formulated by the less spectacular co-operation of an international body of theoretical students represented in the British Isles by the Institute and its parent Societies.

The Ethnological and Anthropological Societies of London and their joint offspring, the Institute, provided the principal medium in Great Britain for the systematic classification of a medley of curiosities and often ill-digested observations, for the conversion of mere speculations into scientific hypotheses, and for the translation of idealistic ventures

into practicable plans. Discussions at meetings, the perusal of the selected communications printed in the Transactions, the Memoirs and the Journal, and the exchange of information with kindred institutions abroad (the Ethnological Society was the natural channel of communication with the Anthropological Society of Paris, already existing in 1843, and with those subsequently formed in Berlin, Vienna, and other capitals), corrected individual eccentricities, cancelled out errors, and checked premature judgments. For it is this sort of collaboration, that produces a science, a body of socially accepted truths and methods based upon pooled experience. Of course, too, it was good for the ethnographers to be continually reminded by the archaeologists that even these ultra-civilized islands had been inhabited only a few thousand years ago by barbarous Stone Age folk. At the same time ethnographers helped the antiquaries to understand how prehistoric relics from British soil were made and used, and so helped archaeology to secure properly classified material, the precondition of becoming a genuine science.

For the Institute and its parents never forgot that the field of its activity included ' all phases in humanity's history and progress.' Indeed it was the Anthropological Society ' of London ' that in 1865 sponsored and financed Dr Joseph Anderson's celebrated excavations in Caithness that have become the model for scientific archaeological work in Scotland. Lord Abercromby's paper of 1902 that changed the whole direction of prehistory in Great Britain was read to the Anthropological Institute.

Finally, the Institute has maintained the international character of the science of man. It took a leading part in the foundation of the new International Congresses of Prehistoric and Protohistoric and of Anthropological and Ethnographic Sciences that first met in London in 1932 and 1936 respectively. In so doing it has always represented the British Isles as a whole even when ' London ' was attached to the names of the constituent Societies. So the Scottish first president of the Ethnological Society was followed by John Crawford from Islay, who was its guiding genius from 1847 to 1869. In the sequel Sir Everard im Thurn and Sir Arthur Keith occupied the Presidential Chair in succession during the last world war. Samuel Laing in the sixties, John Abercromby, David MacRitchie, Robert Munro, R. W. Reid, and William Turner in the eighties had been leading fellows, and the Institute's annual Huxley Lectures have been delivered by Frazer, Keith, and Sayce as well as by outstanding American and Continental scholars.

V. Gordon Childe.

ANTIQUITY, No. 66

Through loss of mails by sea it appears that some of our Subscribers have not received the June (1943) *number. If any Subscribers are disposed to return their copy the Editors will gladly refund the cost, and so be enabled to send to some of those abroad who did not receive it. Such copies should be sent to* 24 *Parkend Road, Gloucester.*

Reviews

TIME AND CHANCE: the Story of Arthur Evans and his Forebears. *By* JOAN EVANS. *Longmans, Green & Co.* 1943. 21*s.*

Reviewing a biography is a delicate task; and it is nearly always a difficult one. But it is less difficult in the present case because the work has been performed so well. The biographer, as it seems to the reviewer, has succeeded in writing a completely satisfactory biography. The book is very well composed and written; sympathy and detachment are in just the right proportion; and not only the central figure but also all the other characters are seen as real people. One does not hurry over the earlier pages, but reads them with genuine pleasure for their own sake. The book, indeed, has a certain rhythm in its composition, as a work of art should. One is tempted to continue in this strain, but let what has been said suffice.

To archaeologists of today Sir Arthur's father, Sir John Evans, is a rather shadowy figure, remembered chiefly as the author of two learned and scholarly books that have become classics. One realizes after reading this book what a great man he was, and what an attractive character as well: and one feels that one would like to have known him. One forgets that the patriarchs were also pioneers in the heroic age of their youth. Sir John Evans was the first to establish the existence of palaeolithic remains in England, after a visit to see the discoveries of Boucher de Perthes in the Somme gravels. He was the first to place the prehistoric British coins in their proper historical setting. He produced the first classification of bronze implements of the Bronze Age, which he divided into periods based on types. The results of his labours have been so completely assimilated that we are unconscious of them; one takes them for granted, just as one does the house one lives in, forgetting that it once had to be designed and built on a vacant site.

The fame of his son rests mainly of course on Crete; but when he first visited that island in 1894 he was already 43 years of age, with a reputation securely established by his researches in Britain and the Balkans. In the middle nineties his active interests ranged over the wide field of Celtic Art (the Rhind Lectures), prehistoric and Roman remains near Oxford, and the primitive Aegean scripts; shortly before he had already, by his article on the Aylesford cemetery, created the La Tène period in England. Thus he had that wide general knowledge that is unfortunately becoming rarer and rarer nowadays, to the great detriment both of archaeology and of archaeologists themselves. He also had that other indispensable—a first-hand experience of life in primitive, 'prehistoric' communities; to one with his sensitive imagination the early adventures in Illyria were an excellent preparation for the task of reconstructing the past. Thus, when he came to Crete, he was able to exploit to the full the great gifts of Time and Chance. It is, we think, a little less than the whole truth to say (p. 338) that he 'had come to the site [Knossos] in the hope of finding a seal impression and a clay tablet, and Time and Chance had led him to discover a civilization'. Surely it was the intimations of genius that led him there—those elusive clues on the mainland must have suggested to him that something great lay somewhere behind; and to this his unerring intuition did in fact lead him. But we must not press the epigram too hard; its author probably means that in searching for seals and tablets, he was searching too for the civilization that they postulate.

In assessing the greatness of the discovery—one of the outstanding ones of all time,

one is tempted to speculate about the circumstances surrounding it; and indeed such speculations are made relevant by the excellent title of the book itself. What would have happened if Time and Chance had not encountered the Man? The answer is easier than usual. The Minoan civilization would eventually have come to light by some other means. But instead of a first-rate site, acquired, excavated (largely at his own expense), reconstructed and fully described by a man of genius, we might have had piece-meal revelation by second-rate men of many nations. We should not have had the volumes of 'The Palace of Minos', but a series of excavators' reports; instead of a symphony we should have got a collection of songs of varying quality.

Sir Arthur Evans has yet another lasting monument of his greatness—he was the effective modern founder of the Ashmolean Museum, which the University of Oxford had allowed to remain derelict for two hundred years. It was for him a most remarkable achievement, for it needed many qualities which he did not seem to possess. In a couple of decades he raised it from an obscure and dusty collection of curios to the finest museum of its kind in the world; and he did so in the face of continual factious opposition from the University authorities themselves. This opposition he eventually wore down; but in the process he learnt 'how curious was [Oxford's] attitude to anything that was neither books nor money' (p. 267). Its own attitude to him had already been made clear when it had refused him fellowships ten years before (p. 182); yet in spite of these rebuffs 'Oxford seemed, after Ragusa, the place in which he felt most at home' (p. 261). Perhaps that was because, though primarily a finishing school for sahibs, Oxford does possess and instil a real respect for learning and the past. It does so by means of the Bodleian (which attracts scholars from everywhere), the professors, the University Press, and the college buildings themselves which create an atmosphere. To these assets Evans added a great museum. One criticizes Oxford, and one regrets that its conception of general culture should still belong to the 18th century rather than the 20th; but after all a narrow concept, effectively inculcated, is much better than none at all; and it is not till one associates with people who have no respect for such things that one realizes how great is the benefit thus conferred on a few by residence in places like Oxford. The trouble was that the Oxford of the seventies and eighties could not see that Evans was the rightful heir of its own best traditions.

In conclusion we cannot forbear quoting the concluding paragraph of Chapter 18, which seems to us to give an admirably just description of the character of this enigmatic man:—

'. . . Unusualness was of the essence of his being. The difference between him and his father is partly expressed in that very fact. John Evans was a man of remarkable consistency, and Arthur a man of paradox. He was flamboyant, and oddly modest, dignified and loveably ridiculous; imperious, and surprisingly gentle; extravagant, yet by no means self-indulgent and in some things austere. He could be as subtle as an Oriental, and simple as a child. He could be fantastically kind, and fundamentally uninterested in other people; he could be fantastically generous, and extremely self-centred. He could be a princely host, and live for months as a recluse; he could be, and was, a devoted worker, and be at the same time both idle and ineffective. He was always loyal to his friends, and never gave up something he had set his heart on for the sake of someone he loved. He was always true to his principles, and always true, at the same time, to his own unconscious sense of the pre-eminent importance of the workings of his own mind. This, indeed, is the explanation of his elusiveness, for he lived as the genius he was; and a genius is a man whose mind works in so unusual a fashion that his truth to that vital working must be the only criterion of his life'. O.G.S.C.

NECROLYNTHIA: a Study in Greek Burial Customs and Anthropology (Excavations at Olynthus, part XI). *By* DAVID M. ROBINSON. (The Johns Hopkins University Studies in Archaeology, no. 32). *Baltimore: Johns Hopkins Press: London: Humphrey Milford.* 1942. *pp.* XXVIII, 280, *and* 71 *plates.* £4 10*s.*

The excavations directed by Prof. Robinson at Olynthus have already provided the first archaeological picture of a Hellenic house in the Aegean at the Classical period, and the clearest available account of the lay-out of a Greek city of the golden age. Now they have provided also the most detailed and exact description of the graves and burial-rites of the same period. Hitherto one had had to rely mainly on Orsi's exhaustive reports on the cemeteries of the Greek colonies, in South Italy and Sicily, imperfectly supplemented by fragmentary data from a few sites in Old Greece and the Islands (notably Rhodes), if one wished to establish comparisons with the better known burials of the Bronze Age and Archaic Iron Age.

The author here describes fully 598 graves containing 644 burials. While admittedly many graves have been destroyed or not been found, the total area of the cemeteries can be reckoned at some 30 acres. That would give ample room for some 17000–18000 burials between 432 and 348 B.C., assuming for that period a population of 10,000 on the basis of the estimated number of houses—1100–1200.

Of the 600 odd graves, 147 were pot-burials of infants (*enchytrismoi*), only 53 contained cremations, the rest extended skeletons either in wooden coffins or under tiles or ' unprotected ' or, very exceptionally, in stone sarcophagi. The cremations had taken place in the graves and were sometimes so incomplete that the disposition of the skeleton on the pyre could be clearly seen and photographed; the ashes were only exceptionally —and that incompletely—inurned. Children were burned as well as adults. And the furniture of cremation graves ' was not inferior in quality nor less abundant than in any type of inhumation burial '. The orientation of all identifiable skeletons has been carefully tabulated; out of 360 inhumations, 216 lay with their heads to the east and feet to the west.

Though just 60 per cent. of all graves contained furniture, it was generally poor. Pots were of course the commonest articles found with the corpses; in addition there were ornaments or mirrors in 68 graves, a couple of small coins in 66 (all probably later than 400 B.C.), terracotta figurines in 60, strigils in 50, astragali in 41, iron knives in 4, loom-weights in 2 and lamps in 2. Infants' burials were often quite richly furnished though not all the grave-goods were appropriate to the age of the deceased. Weapons—iron spear-heads—accompanied only two skeletons, both from curious mass-burials comprising 42 corpses. The only trace of precious metals were a couple of gilt leaves from a bronze wreath.

The grave-goods are not individually described or illustrated here but in the volumes dealing with coins, figurines, metal work, etc. But their general character is discussed in a scholarly manner and compared with that at other Greek cemeteries as are the grave forms and rites. Various speculations as to the reasons for cremation are duly cited but treated with proper reserve. The infants' pot-burials are made the pretext for a long but instructive excursus on exposure which Robinson thinks was less general than commentators have inferred from the literary references. The author rightly insists that the relative scarcity of grave-goods is no proof of society's poverty; an equal scarcity is noticeable in the cemeteries of Athens and Syracuse at the times of those cities' greatest prosperity. He might have gone on to note that it is a general phenomenon in stable societies—even for instance in Bronze Age Britain. It would also have been worth insisting on the total absence of craft and agricultural implements; for this too is a

general phenomenon that Olynthus illustrates nicely (knives of course are not industrial tools).

The skeletons were unfortunately in a poor state of preservation so that only nine subjects could be rescued for anthropological study. Accordingly Olynthus provides no basis for estimates of the average age and expectation of life in the population of a Greek city-state. But some such statistics from a city of the Ancient World are badly needed for comparison with those for European savages and barbarians worked out by Prof. Vallois, and for the Bronze Age barbarians of Hissar in Iran published by Krogman. Still less can nine skulls serve as the basis for a biometric analysis of the population of a classical Greek city. But we should be thankful to find any account at all of the anthropological types represented in such a population, since most excavators in classical lands have been criminally negligent of the skeletal material they dig up.

Here in a long appendix Mr J. L. Angel gives detailed measurements of all his nine skulls and compares them with six basal types which he has deduced from a large series of Greek skulls ranging in age from 3000 B.C. to A.D. 1300. One (no. 1) is a fairly good example of Coon's ' Megalithic ' type and a second (9) approximates thereto. Nos. 4 and 8 are Alpine though with suggestions of Nordic and Mediterranean admixture, while no. 6 comes even nearer to the ' Beaker ' type of Britain. Nos. 3 and 7, both female, are Mediterranean. The statures of males vary from 5 ft. 3 ins. to 5 ft. 6½ ins. As a preliminary to his analysis of these classical skulls, Angel describes for the first time the skeleton dug up by Heurtley some ten years ago from the Neolithic B level of Servia ; it belongs to a very short (barely 5 ft. tall) young male whose skull exhibits ' a maze of disharmonies somewhat more subtly joined than the combination of short Alpine braincase and very long Mediterranean face which is most typical of the Dinaric '.

For all his grateful appreciation of the careful measurements and learned discussion embodied in the thirty pages of this anthropological appendix, the reviewer is still left with the impression that anthropometry has very little to say as to the origin of individuals.

V. G. CHILDE.

LATE EGYPTIAN AND COPTIC ART : an introduction to the collections in the Brooklyn Museum. *Brooklyn Museum : Brooklyn Institute of Arts and Sciences.* 1943. *pp.* 24 *with* 54 *plates. Paper bound* 1 *dollar.*

The collection of late Egyptian and Coptic works in the Brooklyn Museum has grown considerably in recent years and an adjacent library contains most of the published material on Coptic civilization. This publication has been prepared to give visitors to the museum a good survey of the subject ; its object is primarily educational and this object it is admirably adapted to fulfil. The book contains a general introduction by Mr John D. Cooney, 54 half-tone plates and brief notes on the plates with references to the relevant literature which have to be supplemented, however, from an earlier Brooklyn publication entitled *Pagan and Christian Egypt.*

The collection now includes some excellent portraits from the Faiyum (pls. 1–3), a strong marble head of the Constantinian period (pl. 10), a series of characteristic Coptic carvings in limestone (pls. 13–21), some bronzes and a large number of textiles (pls. 38–54), to mention the more interesting exhibits. The illustrations of the carvings are clear and the originals should enable visitors who cannot go to Cairo or Alexandria to get a tolerably good idea of the major phases of sub-antique art in Coptic Egypt, the so-called soft and hard styles. Among the textiles too there are some pieces of the highest quality ; particularly fine is a tunic shown on pls. 49, 50 with a motive frequently repeated which looks as if it were derived from a representation of Gilgamesh—the editor thinks

it may represent Alexander carried to heaven by griffins; the fantastic floral design on the top or left of pl. 45 which is assigned to the 5th or 6th century reminds me of the patterns on Umayyad mosaics and carvings of the 7th or 8th century; two interesting fragments (pl. 54), assigned to the 7th or 8th century, foreshadow late carpet developments.

The introduction was written under difficulties after the editor had been called up to the Army—' inducted ' is the American word—and it is for this reason perhaps the least satisfactory part of the book. It would have been a difficult task in the most favourable conditions to write a short introduction to late art in Egypt: the dating of much of the material is quite uncertain and the most diverse judgments and theories of development have been put forth. The editor has tried to embrace too much. J. W. CROWFOOT.

HESPERIA, Supplement VI: THE SACRED GERUSIA. *By* JAMES H. OLIVER. *American School of Classical Studies at Athens.* 1941. *pp.* XII, 204. 5 *dollars.*

This, like previous supplements, is a corollary to the American excavations in the Athenian Agora, since it centres on some fragments of inscriptions discovered there. The restoration and interpretation of these inscriptions form a considerable portion of the book, but the most important part is the reconstruction of the history of the Gerusia at Athens, an institution of which hitherto very little has been known. Mr Oliver distinguishes two kinds of Gerusia in imperial times—the Dorian or political type, and the Ionic, which, originating in a social club of elderly citizens of high standing, in some cases obtained control of religious celebrations. Such an institution is known to have existed at Ephesus as early as 302 B.C., and from documents here quoted, dating from the time of Marcus Aurelius and Commodus, it appears that a similar institution came into being at Athens a little before A.D. 179, encouraged by the Emperors, who wished to foster orthodox state-religion. This body never usurped political authority, but was definitely associated with certain religious activities. It seems not to have interfered with the Eleusinian Mysteries, which were still controlled by the Eumolpidae, but it was undoubtedly responsible for the organization of the Panathenaic festival.

J. F. DOBSON.

THE ATHENIAN ARCHON LIST IN THE LIGHT OF RECENT DISCOVERIES. *By* WILLIAM BELL DINSMOOR. *Columbia University Press.* 1939. *pp.* XVI, 274. 30*s.*

The subject of this volume is dull and prosaic in itself, but the work done is important in relation to the chronology of events at Athens in Hellenistic times. Inscriptions throwing light on political, social and economic developments may be dated, when the authority of ancient historians fails us, on the evidence here supplied. In 1931 Professor Dinsmoor published *The Archons of Athens in the Hellenistic Age*, and the present work is a re-study of the subject due to the material collected during eight years of excavation in the Agora. The period covered is the 3rd and 2nd centuries B.C.; 16 new names have been added, and there now remain only 20 blank years. In addition to names of archons we have in many cases the name of the secretary and the priest of Asclepios. The subject is complicated by the variation in the number of tribes—at one time as many as 13 at Athens—so that great care must be taken in the application of Ferguson's law of tribal rotation to the assignment of a particular year to each archon whose tribe is known. Some of the peculiar complications of the Athenian Calendar are dealt with in a separate chapter. J. F. DOBSON.

Index

INDEX

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